THE PROCEEDINGS

OF

THE OPTICAL CONVENTION

London, 1905

No. I

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OF

THE OPTICAL CONVENTION

HELD AT THE NORTHAMPTON INSTITUTE, LONDON, E.C.

MAY 30TH TO JUNE 3RD, 1905

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OPTICAL CONVENTION

1905

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TUESDAY, MAY 30th.

INAUGURAL ADDRESS.

By the President, R. T. GLAZEBROOK, D.Sc., F.R.S.

FOR the past six months I have been met frequently by the enquiry, " What is the Optical Convention? what are its aims, and what the purposes it intends to effect?"

Let me take this opportunity of repeating once more what I consider are the answers to these and similar questions, and of enumerating some of the results which I trust will follow from our Meeting.

We aim at increasing the welfare and prosperity of our country by rendering the British Optical Trade more efficient. We are striving for no mere selfish end. The prosperity of the nation is indis- solubly bound up with the prosperity of each of its important trades and manufactures. If Britain is to retain her place in the markets of the world, it must be by the united efforts of all, and we are bound to take our share in the struggle. Our aim is an Imperial one, though maybe in a small way, and it is from this standpoint that I invite you to contemplate our work.

The history of the movement can be briefly stated. Some two years since the suggestion was made by some prominent members of the Optical Society that a Conference of Opticians should be held in London to discuss questions of interest, and to promote the general welfare of the Trade. A preliminary enquiry met with an encouraging response, and in April last year a representative meeting was held at the Society of Arts to consider the question. Resolutions approving the holding of the Convention and appointing Committees were agreed to unanimously, and since then the work has gone forward with the result you have before you to-day. For, though the first idea was merely to hold a Conference for discussion, it was made clear at the meeting that this alone would not suffice, and that an Exhibition and a Catalogue must be added if we wished to make the work complete. The Exhibition enables our members to see what British opticians have done; the Catalogue, we hope, will be of permanent service to many who cannot visit the Exhibition, as well as to those whom we shall welcome here in the course of this week.

The scheme of the Catalogue is not new. In 1900, at the Paris Exhibition, there was published the Special Catalogue of the Joint Exhibition of the German Mechanicians and Opticians. Last year there was a similar German Catalogue at St. Louis, while in 1901-2 the Committee of Manufacturers of optical instruments and of instruments of precision of Paris published a similar list.

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Ours is a Catalogue of the British Optical Trade, and there are few important firms which are not represented in our list.   The Exhibition, like the Catalogue, which is not strictly limited to goods shown, is arranged to illustrate the Trade as a whole.   The instruments are divided into a series of Classes, and while as far as possible each maker's goods in any given Class are placed together, many makers are represented in several Classes.

To the entries under each Class in this Catalogue an introductory statement has been prefixed, and the Committee have been fortunate in the Sub-Editors who have made themselves responsible for this part of the work ; each section, as far as possible, has been committed to two gentlemen, the one a practical member of the Trade, and the other a man with some special knowledge of the theory of the instruments in the Class.

I do not propose to go in detail through the various exhibits.   I should like, however, to call special attention to Mr Dunscombe's interesting historical exhibition of spectacles.

Turning now to the meetings and discussions, our list of papers shows the wide range we cover and the importance of our work.

I trust the outcome of our deliberations may bring into even greater prominence the advantage which must follow from the union of science and practice, and may lead to a fuller realisation of the fact that the day of empiricism in manufacture is over, and that real progress can be expected to follow only when the guiding hand of Science is carefully watched, and the path she indicates boldly followed. For this is the lesson it is most necessary for us to learn, a lesson we are slow to grasp, but one whose consequences, when once we appreciate them, will be far-reaching.

The study of Optics is a fascinating one and its history full of interest.   I do not propose to-night to attempt to cover the whole ground, but to ask you to look at one or two special periods during which it seems to me theory and practice reacted on each other in a marked manner, and to consider what lessons we may draw as to the relation which should in these days of ours subsist between the two. And for this purpose, as those of us who listened lately to Prof. Thompson's address at the Optical Society will realise, I might go back to very early days.   Ptolemy in his attempt to discover the laws of refraction—and wonderfully good the attempt was, as we know now—Archimedes with his burning glass, if indeed he ever made it—had both practical aims in view.

But we will start to-night nearer our own time.   The end of the seventeenth century is such a period. The telescope was invented about 1608, the microscope at rather an earlier date—about 1590—both probably in Holland.   Galileo, hearing of this, made his first telescope in 1610.   In 1611, Kepler, in his *Dioptrica*, describes the astronomical telescope with one or more convex lenses as the eye-piece. With this exception, up to Descartes' book on "Dioptrics" in 1637, no other form of telescope but Galileo's was known.   The law of refraction was first enunciated by Snell in 1621.

Thus by the year 1660 the importance of the telescope to the astronomer was fully appreciated, and its limitations were being realised.   In 1663 Gregory published an account of the first reflecting telescope designed to meet some of these defects, and about this time two men whose work has left indelible marks on the science were led to study it in a great measure from their interest in astronomy—Christian Huyghens, who lived from 1629-1695, and Isaac Newton, 1642-1727.

Huyghens was the discoverer of the wave-theory and of the law of double refraction, but he was also a skilled mechanic, and he worked himself at grinding his lenses and erecting his telescopes.   He realised from a consideration of the theory that many of the most marked defects were due to the fact that the rays from a distant star traversing the various parts of the lens were not brought to a focus at the same point on the axis, and that for a lens of given aperture this axial aberration decreased rapidly as the focal length increased.   The magnification of the telescope depends on the ratio of the focal length of the object glass to that of the eye-piece.   Hence by keeping this ratio constant and increasing both focal lengths in the same proportion the magnification could be maintained, and the spherical aberration decreased.

Thus he was led to make lenses of 120 feet focal length.   Tubes for such instruments could not be

produced, and they were mounted on the top of tall poles and moved from below by ropes. With one of these telescopes, which he afterwards presented to the Royal Society, he discovered Saturn's rings and its fourth satellite. In this case the desire to improve an instrument caused an appeal to theory, and theory led the optician to make a real advance. The advance, it was true, was an inconvenient one, and the defects, as we shall see, were not entirely due to spherical aberration, but the fact remains.

In another branch of instrument-making Huyghens is famous for applying Science to manufacture. His treatise, *Horologium Oscillatorium*, which discussed most ably many problems of motion, was long the standard work on clocks, and he was the first to bring into practical use in 1657 the pendulum as a regulator for time measurements, though, according to Sir E. Beckett, the first pendulum clock actually made was constructed in 1621 by Harris of London, for St Paul's Church, Covent Garden.

In 1665 a posthumous work of an Italian Jesuit, Francis Maria Grimaldi, entitled *Physico Mathesis de Lumine Coloribus et Iride aliisque annexis*, was published at Bologna. It contains some notable observations, particularly the discovery of diffraction.

Newton, who in the previous year had taken his B.A. degree at Cambridge, purchased a prism at Stourbridge Fair in 1666 "to try therewith the celebrated phenomena of colours," and to repeat some of Grimaldi's experiments. During that year also he had applied himself to the grinding of "optic glasses of other figures than spherical." He was already interested in Astronomy, possibly had already made but not confirmed his great discovery. Writing to Halley in 1686 about some of the controversies which followed the publication of the *Principia*, he says: "But for the duplicate proportion, I gathered it from Kepler's theorem about twenty years ago."

The celebrated apple is supposed to have fallen in his mother's garden at Woolsthorpe in Lincoln-shire in 1665, where he was driven by the Plague, and the story has some authority. It is stated to be the fact by Conduitt, the husband of Newton's favourite niece; it was told by Mrs Conduitt to Voltaire, and the tree from which it was said to have fallen was seen by Sir David Brewster in 1820.

Various suggestions have been made for the reason why the discovery that the same cause which produced the apple's fall also maintained the moon in her orbit was not published for many years. The true one is probably due to Professor Adams, who, as Dr Glaisher has told us, pointed out that it was necessary to know the attraction, not merely between two particles of matter, but between two spherical bodies of large size, and that this problem was not solved till much later; but be this as it may, we are sure that in 1667 Newton was an astronomer, and realised the necessity for accurate astro-nomical observations, and all that the improvement of the telescope meant to astronomers.

Now his experiments with the prism in 1666 led to the discovery of the spectrum. Little was known about colours at that time, and Dr Barrow's *Treatise on Optics*, published with Newton's help in 1669, contains very erroneous views; but some time shortly after that date Newton was able to draw the important conclusion that white light is not homogeneous, but consists of rays, some of which are more refrangible than others. The pictures of the spectrum, so familiar to us in numerous text-books, come from Newton's *Optics*, published first in 1704, though his discoveries as to the analysis of white light were laid before the Royal Society in various papers in 1671, and were given in lectures on Optics as Lucasian Professor in Cambridge in 1669, 1670, and 1671.

The bearing of all these physical experiments and researches on the practical manufacture of the telescope was at once obvious; the lenses behave like prisms, and decompose the light into its constituent colours. No alteration of shape will remove this entirely; and Newton was driven, too hastily as we know now, to the conclusion that the refracting telescope could not be greatly improved; its defects were inherent in the refraction of light.

The defect, however, does not exist in images formed by reflection, and he came to the conclusion that optical instruments might be brought to any degree of perfection imaginable, provided a reflecting surface could be found which would polish as freely as glass, and reflect as much light as glass transmits, and provided a method of communicating to it a parabolic figure could be found. In 1668 he thought of a delicate method of polishing, by which he believed "the figure would be corrected to the last," and

the Newtonian reflecting telescope was the result. An instrument made with his own hands is now in the possession of the Royal Society, and the many noble instruments, which have added so greatly to advance our knowledge of the stars, are the direct outcome of Newton's experiments with the prism, and the deductions he drew from them.

But these experiments convey another lesson, for Newton, misled by his observations on dispersion, decided wrongly, as we know now, that achromatic lenses were impossible, and that the colour defects must always exist in refracting instruments, and as a result, attempts to improve these instruments were almost in abeyance for nearly ninety years. Two or three achromatic telescopes were made by Mr Hall about 1730, but it was not till 1757 that Dollond reinvented the achromatic lens, and commenced the regular construction of such lenses.

Thus the discoveries of Huyghens and of Newton reacted powerfully on the instruments of their day. Indeed, in each of these two instances the discoverer and the instrument-maker were the same person. Such a combination may be less possible now, still there are mathematicians skilled in the theory of optics, and opticians skilled in the practice of their art.

The Optical Convention aims at co-ordinating the efforts of the two. But if two hundred years ago the progress of the telescope was determined by the advance of optical theory, theory itself was no less indebted to the interest in instruments and observations thus aroused for the progress that took place.

Huyghens was the founder of the wave-theory, though the labours of Young and the genius of Fresnel had to be before Newton's rival theory of emission was displaced.

For nearly one hundred years after the date of Newton's *Optics* progress was slow. The world was occupied in assimilating what he had taught. English mathematicians, overawed perhaps by his transcendent greatness, employed themselves in expounding his teaching. In England, at any rate, the emission theory was supreme, and few, if any, questioned his dicta as to the impossibility of achromatism.

But a change came with the new century. Thomas Young, (1773-1829), was the first in his various papers between 1801 and 1811 again to call attention to Huyghens' work, and to place on a firmer basis the ground-work of the wave-theory. He it was who established clearly the principle of the super-position of waves, and showed how interference may be explained by it.

Young's work, however, would have been incomplete without Fresnel (1788-1827), who re-discovered for himself the principle of interference, and extended it to explain diffraction, besides enunciating his theory of double refraction and deducing the well-known expressions for the intensity of the light reflected from or transmitted by a transparent surface. Young, in his Lectures on Natural Philosophy, illustrates in an admirable way the applications of optical theory to instruments. Fresnel was an engineer by profession, attached to the service of the bridges and roads, and as such was the inventor of the arrangements of lenses employed in the French lighthouses.

The discoveries of these two men changed the whole of the theory on which the construction of optical instruments is based ; it is idle to attempt to explain the action of a microscope, the resolution of a double star or of the fine lines of the spectrum, to discuss the conditions for such resolution, or indeed, to attempt the construction of any of the more delicate of the beautiful apparatus about us in this room without clearly understanding the fundamental laws discovered by these two, and verified with marvellous skill by Fresnel in his country home in Normandy, not by the aid of modern apparatus, but by such means as his own hands, aided by the skill of the village blacksmith, could construct ; and though it is true that only recently have we appreciated the full importance of the wave-theory in its bearing on the construction of optical instruments, it is the fact that without their labours, and the work of those who followed in their path, few of the modern discoveries of the astronomer, few of the results which the skilled optician of to-day has arrived at, would have been possible. The object glass of a microscope, the lens of a camera or a telescope have reached their present perfection because men have been found who could apply to the art of lens-grinding the highest teaching of Young and of Fresnel.

In the earlier years of the last century, Englishmen were well to the fore in this work. In astronomy

the labours of the two Herschells are well known, and though perhaps the success of the elder Herschell was due rather to his mechanical skill than to a profound knowledge of optical theory, Sir John Herschell advanced in no small measure the application of theory to practice.

At a somewhat earlier date, Fraunhofer of Munich (1787-1826), a contemporary of Young and of Fresnel, had realised the fact that the development of the achromatic lens "depended on the exact determination of refractive indices, and that the chief difficulty in that determination lay in the difficulty of obtaining homogeneous radiations to serve as standards."[1] For these he used the dark lines of the solar spectrum, originally observed by Wollaston, and in this we have an example of the manner in which practical needs react to assist in the advance of Science, for from these observations springs the whole of spectrum analysis and all that is involved in it.

Thus theory and practice progress together, each alone carries us but a short way ; but the judicious use of hypothesis and reason, supported by the verdict of experiment, carries us on to new knowledge and brings us nearer to the truth.

Until after the middle of last century we in Britain took our full share in promoting this advance. We might add to the names already mentioned those of Sir George Airy and of the distinguished men who, in the first half of the century, adorned Trinity College, Dublin, notably Sir William Hamilton.

Sir George Airy gave, about 1802, an account of the aberrations of the lens of the camera obscura, of the utmost value to the early designers of the photographic lens, while Sir William Hamilton's *Essay on the Theory of Systems of Rays* contains the essence of all that is needed to calculate to a high degree of accuracy the aberration of such a lens.

But at that date photographic lenses were not thought of, and when Daguerre announced his invention in 1839, the work of Airy and of Hamilton was forgotten. Thus, to quote again as I did lately in the Traill Taylor lecture, from the recent work of Dr. M. von Rohr :

"The important signification of Airy's writings for photographic optics does not seem to have been appreciated until a later date. Although they exercised an influence on English Text-books, like that of Coddington, they seem, unfortunately, never to have become known in wider circles on the Continent. It appears then that the theoretical opticians of later years to whom his investigations of the astigmatic deformations of oblique pencils would have been of great interest, did not base their work on that of Sir G. B. Airy," while Sir W. Hamilton's paper remained unnoticed by the optician until Finsterwalder called attention to it, and another distinguished German, Prof. Thiessen, quite lately put his results into an accessible form.

There was a divorce between theory and practice in England. The importance of Daguerre's discovery was at once realised, and English opticians set to work with no small success to develop the lens and to make it perfect, and splendidly in many ways they did their task ; but the work was empirical. A certain amount of progress was possible and was achieved, but without the guidance of well-founded theory the progress could not be for long.

The learned transactions of the Cambridge Philosophical Society and of the Royal Society of Dublin were perhaps the last places to which the practical optician would apply for help, and so it came about that because the opticians of another nation first recognised that a full knowledge of the action of a lens on the light that traverses it was a condition precedent to further truth, for some years past the great improvements in the products of the optician's skill which have taken place have had their origin mainly in Germany.

And this brings me to our last example of the manner in which science and practice may combine to produce effects unattainable by either singly. But before dealing with this I would mention one great advantage which, until a few years ago, the English optician possessed in a peculiar degree, an advantage to which much of the progress of our English lenses is undoubtedly due. The story of Guinand's invention of optical glass is deeply interesting. A poor carpenter, and later a watch-case-maker of Brenets in Canton Neuchatel, he was born in 1740, and became at an early age interested in

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[1] Schuster, *Theory of Optics*.

telescopes. Prompted by the desire to possess a pair of spectacles, he undertook to make the glass for the lenses. A little later through M. Droz, a gentleman of the neighbourhood, he was allowed to examine one of Dollond's achromatic lenses, and learnt of the difficulty of obtaining the flint glass required. This he determined to make, and years of penury and unremitting toil followed, till at last he succeeded in casting discs sufficiently homogeneous to be used for optical work. Fraunhofer persuaded him to migrate to Munich, but the venture was not a success. He returned to Switzerland and again started glass-making. After his death his son told the secret of the art to George Bontemps, a Frenchman, who some years later was brought to England by Messrs Chance, and helped them to establish the Optical Glass Works which for so long were practically the sole source of the supply of raw material for the optician.

Our Catalogue to-day bears witness to the progress in glass manufacture that has taken place since Bontemps' time, and it is right to recognise the influence that progress has had on opticians' work.

But to return to our main subject. An Optical Convention in 1905 would be incomplete without some reference to the work of that master optician who a few months ago was taken from us; the more so since the work of Ernst Abbe affords perhaps the most striking illustration of the effects of the reasoned combination of theory and practice.

A comparison of the statistics of the optical trade of Germany now and twenty years ago suffice to prove this. The story of the growth of the Jena industry has been told frequently, still I will repeat it in barest outline.

Abbe, then a young man, had settled at Jena as a Privat Docent in 1863, and soon after Carl Zeiss, who then made microscopes of the ordinary class, applied to him for help in the development of the instrument. Abbe's task was a hard one; the theory of the microscope was at that date only partially understood, the corrections to the lenses were made by a rough trial and error method, and the results were doubtful. The first step was to solve a mathematical problem of no small difficulty, to trace the paths of the pencil through the object glass. Abbe soon realised the defects of the ordinary theory. He found it necessary to apply the principles of the wave-theory, the teaching of Young and Fresnel, to the problem, and was led in 1870 to the theory of microscopic vision, which bears his name. His work was the direct outcome of that of Fresnel.

He soon realised that it followed from the mathematical theory that with the glass then at the optician's disposal no great improvement in the microscope object glass could be expected. Certain relations between the dispersion and refraction in the various lenses were requisite to secure achromatism, and no glass showing these relations existed. An inspection of the instruments in our loan Exhibition at South Kensington in 1876 confirmed this view, and he published it in a Report in 1878 on the results of the Exhibition. "The future of the microscope as regards its future improvement in its dioptric qualities seems to be chiefly in the hands of the glass-maker."

The investigations of Petzval and of Von Seidel led to a similar result with regard to photographic lenses. Von Seidel's work dates back to 1856–57, but his main paper was not written till 1880, after the date of Abbe's report, and was not published in full until 1898.

It follows from these investigations that with the glass then on the market it was impossible to make the field of a photographic lens at once flat and achromatic.

Thus the theoretical work indicated a bar to future progress which could only be removed by the manufacture of new glasses having certain definite properties. It is fitting to say that at an earlier date this fact had been recognised by our countrymen, Mr Vernon Harcourt and Prof. Stokes, who for some eight years previous to 1870 had endeavoured, but with scant success, to make the glass required.

Abbe was more fortunate; his report fell into the hands of Dr Otto Schott, a glass-maker in Witten in Westphalia, who realised its importance. In 1881, Schott communicated with Abbe, and the next year he removed to Jena, and the firm of Schott & Partners was born.

In the first catalogue of the Jena Glass Works they write: "The industrial undertaking here first brought into public notice, and which has arisen out of a scientific investigation into the dependence

between the optical properties and the chemical composition of solid amorphous fluxes which was undertaken by the undersigned (Schott and Abbe) in order to discover the chemico-physical foundations of the behaviour of optical glass, . . .''

The enquiry was aided by large grants from the Prussian Minister of Education. The practical result is seen in the Catalogue of the Jena firm, and the enormous export of German optical goods.

Nor is this all, for in virtue of the distribution of profits settled by the scheme of the Carl Zeiss Stiftung, drawn up by Abbe some years ago, and ratified by the Government, the University of Jena alone has received a sum approaching £100,000.

Abbe's work at Jena is perhaps the most striking illustration of the way in which progress depends on the co-operation of science and experience.

One could give statistics to illustrate the truth of this, and the important effect it has had on German trade and prosperity. They are hardly necessary; the facts are patent, and their cause well known to all who care to enquire.

We, too, can progress if we follow the path laid down for us of old by Newton, Young, Herschell, Airy, and the others of whom I have spoken.

I trust I have answered my questioners, and explained with sufficient clearness the object of those who are chiefly responsible for this undertaking.

The lesson is being learnt; the Exhibition will show that in many branches of our art we fear no rivals. We still possess the qualities of honesty, doggedness, and pluck, which in the past have made England great, and now we are adding to them the practical scientific knowledge of the subject, which in this twentieth century more than ever before is essential to success.

WEDNESDAY, MAY 31*st.*

Dr R. T. GLAZEBROOK, M.A, F.R.S. (President) IN THE CHAIR.

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BIOGRAPHICAL CHART OF SOME OPTICAL WORKERS.

By FREDERIC J. CHESHIRE, F.R.M.S.

THE plan upon which this Chart is drawn is obvious on inspection. The names of men who lived and worked contemporaneously will be found arranged vertically above one another, the duration of the life of each worker being represented by a horizontal line starting at the year of birth and ending at the year of death. A dotted beginning or ending indicates that the corresponding date is more or less uncertain. The plan here adopted of exhibiting graphically, in order of time, the names of eminent workers in the field of Optics was apparently first used by Dr Joseph Priestley in the book written by him on *The History and Present State of Discoveries relating to Vision, Light, and Colours* (London, 1772). The same method was also used freely by the famous Dr Thomas Young in his lectures given at the Royal Institution, and published in 1807 under the title of *A Course of Lectures on Natural Philosophy and the Mechanical Arts.*

An alphabetical list is given below of the names occurring on the Chart. The figures in brackets, giving the year of birth and death, respectively, have been obtained mostly from Poggendorff's *Biographisch-Literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften,* which has just been completed up to the year 1900. The names of living workers are omitted from this list for obvious reasons.

No one, perhaps, is more conscious of the deficiencies of the Chart than its author. Many names, doubtless, have been omitted which should have been inserted, and, on the other hand, some names have, perhaps, been inserted which should have been omitted. The Chart is given simply in the hope that it may be of some use to the student who wishes to take up for the first time the entrancing study of the History of Optics.

Abbe, Ernst (1840-1905).
Airy, George B. (1801-1892).
Alhazen (fl. 1030).
Amici, Giambattista (1786-1863).
Angström, Anders J. (1814-1874).
Arago, Dominique F. J. (1786-1853).
Archimedes (B.C. 287-212).
Aristotle (B.C. 384-322).
Auzout, Adrien (1630-1691).
Bacon, Roger (1214-1294).
Bartholin, Erasmus (1625-1698).
Becquerel, Alexandre E. (1820-1891).
Biot, Jean B. (1774-1862).
Boscovitch, Ruggiero G. (1711-1787).
Bougeur, Pierre (1698-1758).

Boyle, Robert (1627-1691).
Bradley, James (1692-1762).
Brahe, Tycho (1546-1601).
Brewster, David (1781-1868).
Bunsen, Robert W. (1811-1899).
Campani, Giuseppe (fl. 1660).
Cassegrain (fl. 1660).
Cassini, Dominique (1625-1712).
Cauchy, Augustin L. (1789-1857).
Clairaut, Alexis C. (1713-1765).
Clerk-Maxwell, James (1831-1879).
Copernicus, Nicolaus (1473-1543).
Cornu, Marie A. (1841-1902).
Crabtree, William (1610-1644).
Daguerre, Louis J. M. (1789-1851).

Descartes, Réné (1596-1650).
Divini, Eustachio (fl. 1660).
Dollond, John (1706-1761).
Dominis, Marco A. de (1566-1624).
Doppler, Christian (1803-1853).
Empedocles (fl. B.C. 450).
Euclid (fl. B.C. 300).
Euler, Leonhard (1707-1783).
Faraday, Michael (1791-1867).
Fermat, Pierre de (1608-1665).
Fizeau, Armand H. L. (1819-1896).
Foucault, Jean B. L. (1819-1868).
Fraunhofer, Joseph (1787-1826).
Fresnel, Augustin J. (1788-1827).
Galilei, Galileo (1564-1642).

B.C. 900 TO B.C. 200			
	——— ARISTOTLE ———	——— ARCHIMEDES ———	
		---- EUCLID -----	
900	400	300	200

B.C. 200 TO A.D. 500			
200	300	400	500

A.D. 500 TO A.D. 1200			
	-- ALHAZEN ----		
500	1000	1100	1200

A.D. 1200 TO A.D. 1900

1200	1700	1800	1900

——— LISTER ———
——— JAMIN ———
——— FRESNEL ———
——— FRAUNHOFER ———
——— BOUGEUR ——— ——— DAGUERRE ———
——— SCHWERD ———
——— BIOT ———
——— WHEATSTONE ———
——— LIEBERKUHN ——— ——— BREWSTER ———
——— MOOR-HALL ——— ——— HELMHOLTZ ———
——— HERSCHEL J ———
——— HERSCHEL W. ——— ——— HERTZ ———
——— YOUNG ——— — CLERK-MAXWELL ———
——— AIRY ———
HALLEY ——— ARAGO ———
——— BRADLEY ——— ——— STEINHEIL ———
HADLEY ——— ——— STEINHEIL ———
——— DOLLOND ——— ——— HARTNACK ———
——— SHORT ——— ——— KIRCHHOFF ———
——— BUNSEN ———
——— BOSCOVITCH ——— ——— ZEISS ———
——— WOLLASTON ——— ——— ABBE ———
——— CLAIRAUT ——— ——— AMICI ———
——— EULER ——— ——— ANGSTRÖM ———
——— KLINGENSTIERNA ——— ——— BECQUEREL. E. ———
——— CAUCHY ———
——— CORNU ———
——— DOPPLER ———
——— FARADAY ———
——— FIZEAU ———
——— FOUCAULT ———
——— GAUSS ———
——— MUSSCHENBROECK ——— ——— ROWLAND ———
——— RAMSDEN ——— ——— RUTHERFORD ———
——— STOKES ———
——— NICOL ———
——— MALUS ——— ——— SEIDEL ———
——— GREEN ———
——— LITTROW ———
——— PETZVAL ———
——— HAMILTON ———
——— PORRO ———

R.P.

1200	1700	1800	1900

B.C. 900 TO B.C. 200	900	800	700	
B.C. 200 TO A.D. 500	200	B.C. 100	0	
A.D. 500 TO A.D. 1200	500	600	700	
	1200	1300	1400	

----VITELLIO----

————BACON————

——PECKHAM——

—L–LL

A.D. 1200
TO
A.D. 1900

A Biographical Chart

of some Optical Workers.

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Drawn after the manner

of D.ʳ Joseph Priestley by

Frederic J. Cheshire. F.R.M.S.

- - EMPEOCLES - - -   — ARISTOTLE —   — ARCHIMEDES —

600   500   400   300   200

- - - PTOLEMY - - - -

A.D.100   200   300   400   500

- - - ALHAZEN - - - -

800   900   1000   1100   1200

1500   1600   1700   1800   1900

- - - - EUCLID - - - -

LISTER
JAMIN
FERMAT
FRESNEL
DE LA HIRE
FRAUNHOFER
PORTA
BOUGEUR
DAGUERRE
BARTHOLIN
SCHWERD
KIRCHER
BIOT
RHEITA
WHEATSTONE
SCHEINER
LIEBERKUHN
BREWSTER
DESCARTES
MOOR HALL
HELMHOLTZ
LIPPERSHEY
HUYGENS
HERSCHEL J
BAROO DA VINCI
HEVELIUS
HERSCHEL W.
HERTZ
KEPLER
HOOKE
YOUNG
CLERK-MAXWELL
COPERNICUS
GALILEO
NEWTON
AIRY
BRAHE
TORICELLI
HALLEY
ARAGO
MAUROLYCUS
BRADLEY
STEINHEIL
HORROCKS
STEINHEIL
GEMMA-FRISIUS
CRABTREE
HADLEY
HARTNACK
GASCOIGNE
DOLLOND
KIRCHHOFF
DE DOMINIS
AUZOUT
SHORT
BUNSEN
CASSINI
ZEISS
SNELL
BOSCOVITCH
RÖMER
WOLLASTON
ABBE
GREGORY
CLAIRAUT
AMICI
BOYLE
EULER
ANGSTRÖM
- - - CASSEGRAIN - - -
KLINGENSTIERNA
BECQUEREL. E.
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CAUCHY
- - - - DIVINI - - - - -
CORNU
GRIMALD
DOPPLER
LEEUWENHOEK
FARADAY
MARIOTTE
FIZEAU
TSCHIRNHAUSEN
FOUCAULT
GAUSS
MUSSCHENBROECK
ROWLAND
RAMSDEN
RUTHERFORD
STOKES
NICOL
MALUS
SEIDEL
GREEN
LITTROW
PETZVAL
HAMILTON
PORRO

1500   1600   1700   1800   1900

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Gregory, James (1638-1675).
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Hadley, John (1682-1744).
Halley, Edmund (1656-1742).
Hamilton, William R. (1805-1865).
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Helmholtz, Herrmann L. F.(1821-1894).
Herschel, John (1792-1871).
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Kepler, Johann (1571-1630).
Kircher, Athanasius (1601-1680).

Kirchhoff, Gustav R. (1824-1887).
Klingenstierna, Samuel (1698-1765).
La Hire, Philip de (1640-1719).
Leeuwenhoek, Anton van (1632-1723).
Lieherkuhn, Johann N. (1711-1756).
Lippershey, Hans (?-1619).
Littrow, Joseph J. E. von (1781-1840).
Lister, Joseph J. (1786-1869).
Malus, Etienne L. (1775-1812).
Mariotte, Edme (1620-1684).
Maurolycus, Franciscus (1494-1575).
Moor-Hall, Chester (1703-1771).
Musschenbroeck, Pieter von (1692-1761).
Newton, Isaac (1643-1727).
Nicol, William (1768?-1851).
Peckham, John (1240-1292).
Petzval, Joseph (1807-1891).
Porro, Ignazio (1795-1875).
Porta, Giambattista della (1538-1615).
Ptolemy, Claudius (fl. 130).
Ramsden, Jesse (1735-1800).

Rherta, Anton M. S. de (1597-1660).
Römer, Olof (1644-1710).
Rowland, Henry A. (1848-1901).
Rutherford, Lewis M. (1816-1892).
Scheiner, Christoph (1575-1650).
Schwerd, Friedrich (1792-1872).
Seidel, Philip L. (1821-1896).
Short, James (1710-1768).
Snell, Willebrord (1591-1626).
Steinheil, Hugo A. von (1832-1893).
Steinheil, Karl A. von (1801-1870).
Stokes, George G. (1819-1903).
Toricelli, Evangelista (1608-1647).
Tschirnhausen, Ehrenfried von (1651-1708).
Vinci, Leonardo da (1452-1519).
Vitellio (fl. 13th century).
Wheatstone, Charles (1802-1875).
Wollaston, William H. (1766-1828).
Young, Thomas (1773-1829).
Zeiss, Carl (1816-1888).

THE CONSIDERATION OF THE EQUIVALENT PLANES OF OPTICAL INSTRUMENTS.

By Conrad Beck.

The Gauss Planes or the Nodal Planes and principal planes of optical systems are well known to students of optics, and I will not take up your time by discussing their properties at great length. The nodal planes have certain well-known properties. The principal planes have certain other definite properties, but when the medium on both sides of a system of lenses is the same, as is generally the case with optical instruments, these two sets of planes become superimposed, forming one pair having the properties of both. This resultant pair of planes is called the equivalent planes, as opposed to the nodal and principal planes, which are the terms applied to the separate pairs when they have a separate existence in consequence of the two outside media not being the same.

In considering an optical instrument, it is necessary to formulate some system on which to describe its capabilities and action—its power, its aperture, and its various other qualities, so that its suitability for accomplishing any special work may be estimated. To compare one instrument with another, some terms of measurement must be devised. A simple convex lense was probably one of the earliest optical refracting instruments used, and in dealing with a thin single lens, it was found that the distance of its burning point or solar focus formed a measure of its refracting power, and was, in fact, the most important measure that had to be obtained before it could be classified. This measurement, namely the distance of solar or principal focus from the lens, is called its focal length. It was found that the aperture of the lens had to be expressed with relation to the focal length, and that its depth and other qualities were also dependent upon this important distance.

As soon as more complex instruments were considered, it was frequently found possible to find the focus or burning point, but not to find the focal length, because in an instrument, consisting of perhaps half-a-dozen lenses, it was impossible to determine from which lens or from what point such a

focus should be measured. The consequence was that the early English writers preferred to deal with a compound instrument as a series of separate parts, of which they knew the separate optical data, rather than as a whole, and the optical data of the complete instrument were only imperfectly stated. Thus a microscope was investigated as an objective or object lens, and an eyepiece or an eye lens; both in the microscope and the telescope, such a method answered most requirements, but this method became impossible in such apparatus as photographic lenses. The photographer did not wish to know anything about the four or six individual lenses which were used to build up the complete instrument, he required to know the size of picture which the complete lens would produce, and its rapidity, depth of focus, etc. Now for this purpose he must know the true focal length of his instrument as a whole. In the early days of photography the focus was measured either from the back lens or from the diaphragm, but it was soon found that neither of these measurements gave the true focal length, or gave data that could be relied on in practice.

Owing to the neglect of the study of geometrical optics in this country, the correct position from which the focal length should be measured was not universally understood, and text-books talked about an optical centre or a perspective centre from which such measurement should be made. These terms were an approximation to the truth, but the writers who used the expressions were evidently very hazy about them, and could not specify exactly what they meant. Therefore, although the focus of the apparatus was often correctly obtained by indirect methods, and was called the equivalent focus, the fact was not understood that the equivalent focus was merely the focal length of the system, just as the distance from the burning point to the edge of a lens is the focal length of a thin lens, only it had to be measured from the equivalent planes.

The matter had been thoroughly investigated on the Continent fifty years before, and it was discovered that there is no single optical centre or perspective centre to an optical instrument, but a pair of planes, the equivalent planes (or under special conditions two pairs of planes, the nodal and principal planes) from which the optical measurements should be made.

The reason why all measurements must be made from the equivalent planes can be demonstrated immediately by a diagram:
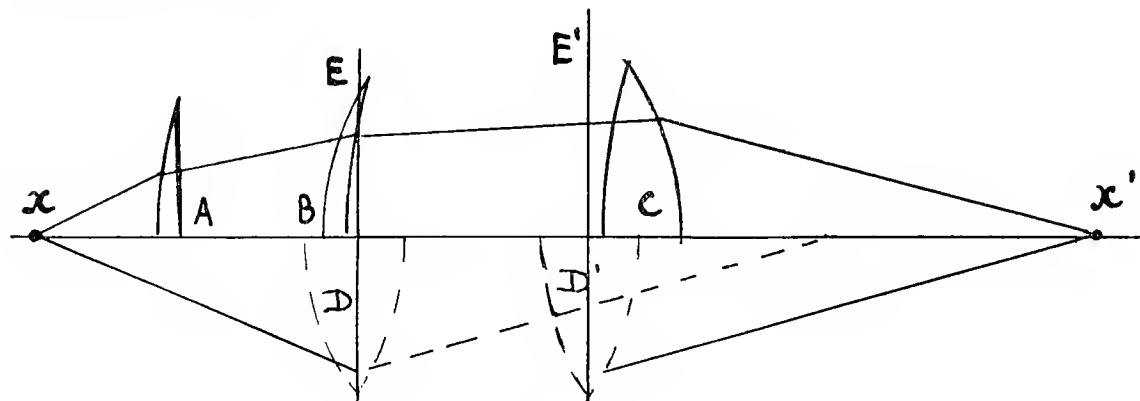
Fig. 1.

Suppose A B and C placed at certain distances apart form a compound optical instrument, for instance, a photographic lens. In the upper half of the diagram the course of a ray of light from x passing through the three lenses to x^1 is shown, each lens bending the light to a certain extent, but we may wish to consider the effect of the three lenses A B and C as a whole. It is impossible to find any single lens that, placed in any one position, will act in a similar way to this compound instrument; if, however, we take a thin single lens D D^1 of a certain focal length, and place it at a position E to receive the rays of light, and then rapidly shift it to E^1 to discharge these rays, it will act in an exactly

similar manner to the compound instrument. Such a lens can be found for all optical systems that have a focus. This lens is called the equivalent lens, and the focus of this lens is the true focus, or the equivalent focus of the compound instrument, and the two positions where the equivalent lens has to be placed, first to receive, and then to discharge the light, are the equivalent planes of this compound instrument.

The top half of the diagram shows how the light is actually refracted, the lower half shows how the light could be refracted by an equivalent lens if it had the power, after it had received a bundle of light, of rushing sufficiently quickly from E to E^1 before it had time to emerge. Consequently, if all measurements with reference to light entering the instrument are made from E, and with reference to light emerging from the instrument are made from E^1, the results will always be correct, as the refraction may be considered as if all took place at these two planes.

I must here qualify this statement by stating that the instrument to which such a method is applied must be considered to be perfectly corrected for aberrations, and to obey the tangent instead of the sine conditions. However, with most good optical instruments they are sufficiently nearly so to render the system correct for general optical problems.

The requirements of the case made it necessary to find the true or equivalent focus of a photographic lens. It has not been so necessary to find the equivalent focus of a complete microscope or a telescope, or to find the positions of their equivalent planes, because if their magnifying power and aperture were determined by experiment, the operator was satisfied. Nevertheless, I hope to point out that the subject, although hitherto neglected, has important bearings on optical investigations, and I propose to indicate the positions of these planes and foci in the chief types of instruments.

In order to investigate the position of the equivalent planes of an optical apparatus, I will first consider two thin single lenses placed at different distances from one another, because one of the factors that largely governs their position is the separation of the refracting portions of a compound instrument, whether these portions be merely two curves of a thick lens or a number of different lenses.

Fig. 2 illustrates the way in which the positions of the equivalent planes vary when a pair of thin lenses are separated to a greater or less extent.

Fig 2.

I must ask you to excuse the size and thickness of the lenses in the Figure. They should be about one quarter the size, but their dimensions do not affect the results which I wish to point out.

In diagram 1 the lenses are separated by a comparatively small space, and the two equivalent planes are situated near the centre of this space. For clearness the first equivalent plane from which all optical measurements on the left of the lenses must be made, is shown in full line marked E, while the second plane is shown as a dotted line marked E^1.

In diagram 2 the lenses are separated by double the distance and the planes have changed their position to a considerable extent. The two planes are crossed; parallel light from the right entering the
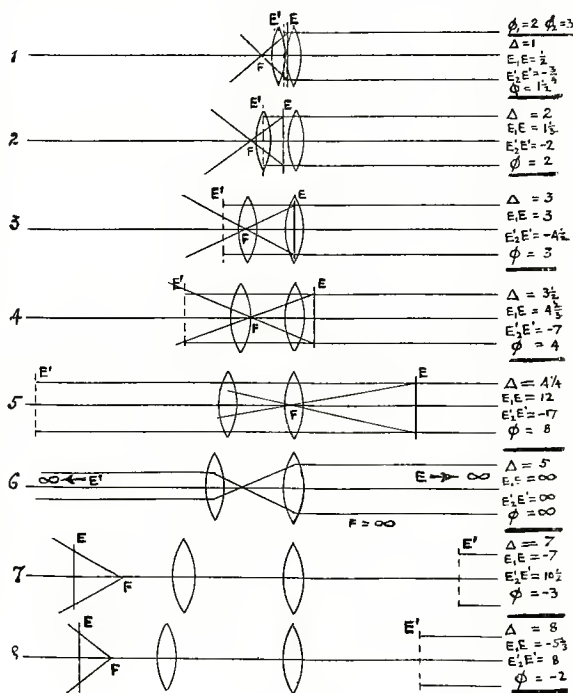
equivalent lens of the system placed at E^1 will emerge from E to a focus F, the focal length of the system being E F.

In diagram 3 these crossed planes have separated to a much greater extent, and the focus is on the centre of the second lens.

In diagrams 4 and 5 the results are still more striking, the focus being virtual, the light emerging from the lenses in a divergent beam from a focus inside the lens system.

Diagram 6 shows the lenses separated to a distance equal to the combined focal lengths of the two lenses. Parallel light, after passing through the first lens, is brought to a focus at the point which is the focus of the second lens, and it is accordingly sent out parallel. In this case the combined pair of lenses has no focus, or we might say its focal length is infinite and the focus is situated at infinity. It will have been noticed that in the successive diagrams, each of which shows the two lenses slightly more separated, the focal length has been getting steadily longer, and at this particular separation of the two lenses the principal focus becomes infinite.

It will also be noticed that the equivalent planes have been getting further away from the lenses, and at this particular point they have also gone to infinity, one on the right- and the other on the left-hand side.

If we carry the separation of the two lenses a little further, as in No. 7, we obtain a focus, but a negative one, and the equivalent planes reappear, the one that vanished to infinity at the right making its entrance from infinity on the left, and E F is now the focal length of the system.

The next diagram (Fig. 3) shows the changes of position of the equivalent planes due to the various separations of a pair of lenses, one of which is positive and the other negative.

I shall refer to these diagrams again, but introduce them here to illustrate how profoundly the construction of a compound optical instrument may be altered by separating its component parts. The focal length and the positions of the equivalent planes are also modified by the relative powers and curvatures of the separate components, but it is not essential to go into these alterations in order to obtain a general conception of the arrangement. By means of two separated lenses, I propose to illustrate the whole series of optical instruments, and to show that such a pair of lenses with different separations forms either a projection lens, a telescope, or a microscope.
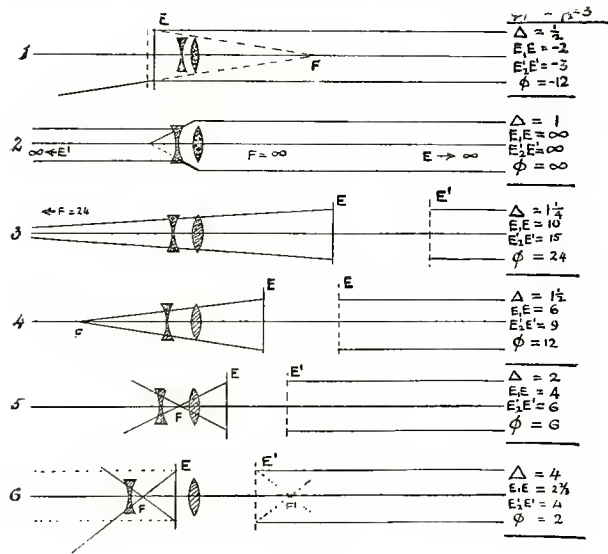
Fig. 3.

Optical instruments which are compounded of lenses, may be almost without exception included under two heads:

(*a*) Projection Apparatus.
(*b*) Appliances to be used with Projection Apparatus.

Projection apparatus include the human eye and also various instruments, such as the photographic lens and the lantern objective which produce actual images.

It is interesting to note how close a similarity exists between the human eye and the photographic camera and lens.

Each has its lens, each has its iris diaphragm, each has its cap or eyelid for opening, each has its dark chamber, and each has its chemical material which is affected by the light.

Now other optical instruments (composed of lenses) are almost without exception adjuncts to the projection apparatus, and are useless by themselves. There are condensers as used in the lantern. There are spectacles as used to aid the eye, and telescopes and microscopes as used to give additional powers to the eye.

I am leaving out of consideration several large classes of instruments such as those that are used for measuring angles or for analysing light because, except in so far as they are constructed of telescopes or microscopes, they do not possess equivalent foci or equivalent planes.

As a type of projection apparatus, we cannot do better than examine the photographic lens, and in Fig. 2 the diagram 1 gives a general idea of the position of the equivalent planes and the focus; the planes are situated between the two lenses generally slightly crossed over. They may vary in position, but except in the special case of a telephoto lens, which will be referred to later, they are always situated fairly near to one or other of the component lenses. When we separate the two lenses as in diagram 6 of the Figure, we have changed the instrument into a telescope, the one lens representing the object glass, the other the eyepiece; both the equivalent planes and the focus have vanished to infinity, but directly we begin to focus our telescope, so that we are looking at near objects, they make their appearance again, although they are situated at a great distance away. On further separating our lenses, as in diagram 8, we obtain a microscope, the object being placed at F and the eye at E^1.

In Fig. 3 we can produce similar results. In diagram 2 we have the telescope of the opera-glass description; by separating it still further we obtain a special projection apparatus of the telephoto lens type, in which the equivalent planes are out in front of the camera; by still further separating the lenses we are returning more nearly to the ordinary photographic lens; and by separating them even more, as in diagram 6, we obtain a microscope of the Brucke form with a positive object glass and negative eyepiece, the eye being placed at the left and the object being placed at F^1 (dotted lines) on the right of the positive lens.

Thus by separating a pair of lenses the whole range of optical instruments can be investigated as to the general arrangement of their focal lengths and equivalent planes, and it will be interesting to consider the advantages that are gained by shifting the position of these planes.

In photographic lenses the chief use that has been made of this property has been the telephoto lens. The scale upon which an object is photographed, whether, for instance, a distant object shall be the size of a pin's head or an orange, depends entirely on the equivalent focus of the photographic lens. For some purposes it is advantageous to have a focus as long as 50 or 100 in., and to avoid the inconvenience of a camera of this length the telephoto lens has been constructed, in which the equivalent planes are situated in front of the camera as in diagram 3. The focus of the lens is long, and the camera short.

I think it quite possible that in the future it may be found advantageous to construct projection lenses in which the component parts are so arranged that the equivalent planes are inside the camera, and are much nearer to the photographic plate than the lens is. The subject might well be investigated to discover if such a plan would offer advantages for special purposes, as, for instance, to obtain lenses of exceptionally large apertures.

With reference to telescopes, as the planes are at infinity we can scarcely spend much time on their investigation until our finite minds have developed into a higher form. Nevertheless, telescopic systems are most interesting, and complete formulæ have been worked out by which their elementary properties can be investigated by simple algebra. The compound microscope is an exceedingly interesting instrument when considered from this point of view.

A microscope of the highest power, considered as a whole, has an equivalent focal length of only a few thousandths of an inch, and the object being examined has to be placed approximately at the focal point. The earliest microscopes were constructed like our pocket magnifiers of single lenses of various curvature,

which may be represented by such lenses as are shown in Fig. 4; the object was placed at F and the eye to the right of the lens. Such lenses could only be made of comparatively small magnifying power,
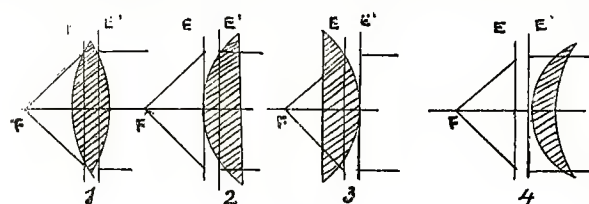
Fig. 4.

and even then the object had to be placed very close to the lens. If we could conceive of a single lens with a magnifying power of 2000 the distance FE would only be $\frac{5}{1000}$ or $\frac{1}{200}$ of an inch, and the object would be so close that it could not even be protected by a thin cover glass. It is, however, interesting to note that lenses of different shapes, although they are single lenses, are suitable to a greater or less extent for increasing this so-called working distance, and that if other considerations rendered such shapes suitable, the lenses Nos. 2 and 4 give greater working distance than Nos. 1 and 3, as in Nos. 2 and 4, due to the shape of the lenses, the equivalent planes are nearer to the object side of the lens than is the case of Nos. 1 and 3. The shape of the lens, although it does not have as much influence upon the position of the equivalent planes as the separation of different lenses, is still a factor of importance.

For obtaining high magnifications, single lenses cannot be made with sufficiently strong curvature, and the first idea that suggests itself is to place three or four powerful lenses close together. Such an arrangement, however, is even more unsuitable, because the equivalent planes are generally somewhere between the lenses, and the actual distance of the focus from the front lens is reduced still further than in the case of a single lens.

Fig. 5 shows a $\frac{1}{8}$ inch microscopic object glass as actually made, and if this lens were upon so small a scale that its focal length FE were only $\frac{5}{1000}$ of an inch, the object to be examined would have to be placed practically upon the surface of the first lens.

So it was, that without knowing the exact reason, the plan of using lenses separated by large intervals was adopted in microscopes as far back as the year 1650, and by examining diagram 6 of Fig. 3 we see the advantage thus gained. The equivalent planes are so placed that one of them, E^1, is at a considerable distance from the lenses, and even if the focal length of the complete instru-

Fig. 5.

ment FE were $\frac{5}{1000}$ of an inch there is ample room for manipulating the object. This arrangement is in actual use in dissecting microscopes where great working distance is of paramount importance, but it has too many disadvantages to make it convenient for the best compound microscopes. The modern compound microscopes consist of two positive lenses separated by a large interval, as in diagram 8, Fig. 2, and here indeed we have a most extraordinary condition.
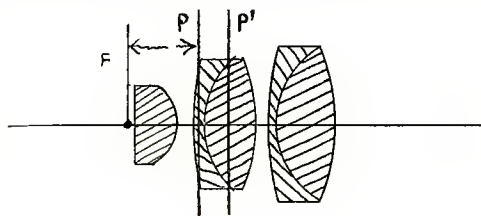
The two equivalent planes are outside everything; the object to be examined is placed at F; the observer's eye is between E^1 and the lenses.

The point, however, upon which I wish to lay stress, and one which I think has been overlooked, is that the positions of equivalent planes of optical instruments, when such instruments are considered as a whole, is the first factor which governs their usefulness for their special purposes. The instruments which have so far been evolved have been developed empirically, the desired results have been obtained by experiment; but now that the elementary theory of lenses is so thoroughly understood, this important question deserves to be carefully investigated. I can well imagine that optical problems may arise, as, for instance, in the invention of gun-sights or range-finders, in which instruments may be required which are neither telescopes nor microscopes of the ordinary type. Such problems should first be investigated with reference to whether the equivalent planes can be placed in entirely new positions more suitable to the

conditions that have to be fulfilled, and it is by no means impossible that by this means a new type of instrument will some day be devised, which would be extremely difficult to invent by ordinary methods.

Before closing this paper, there is another branch of the subject to which I should like to refer, namely, the way in which this method of treating optical instruments as a whole applies to spectacles in combination with the eye. The subject is of such interest that it should be carefully investigated by oculists. In the case of low-power spectacles, the lens that is added to assist the eye, being of but small power compared with that of the eye itself, produces but little effect upon the position of the nodal and principal planes of the eye, but in pronounced Myopia, a very material change is produced.

Fig. 6 shows a diagram of an eye myopic to an extent of 20 dioptrics corrected by a — 20D spectacle lens. The three figures represent the same lens at different distances from the eye; the upper one is closer than would be possible in practice, the second one is at about the usual position, the third one is somewhat too far away.
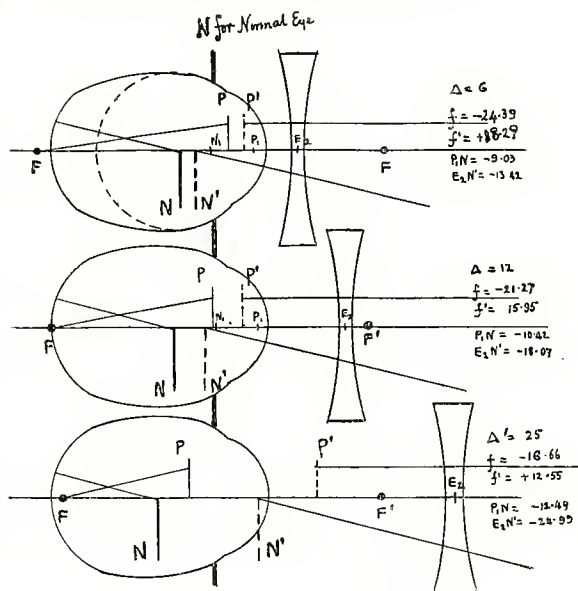
Fig. 6.
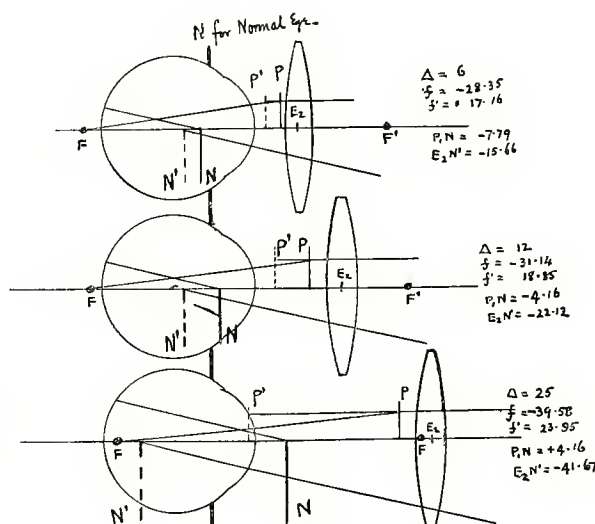
Fig. 7.

The positions of the nodal planes and principal planes for the complete combination of the eye and its correcting lens combined are marked in each figure and show what an influence upon their position is produced by the distance of the spectacle lens from the eye. The dark vertical line indicates the back nodal point of the normal eye.

In examining the central figure one interesting fact is noticeable. Whereas the focal point has been thrown back to the retina by the spectacle lens, the size of the image received upon the interior of the eye is substantially the same, because the nodal plane has been thrown back to an almost similar amount.

The lowest figure shows that in this case the nodal plane is thrown back to a greater extent than the focus, and the image received by this eye is positively smaller than the normal size. The importance of wearing high-power spectacles in an exact position is accentuated when the subject is studied from this point of view.

It may be objected that such extreme myopia is rare, and that the subject, although interesting, has little bearing upon practical ophthalmology; be this as it may, the subject becomes of the greatest interest in cases of cataract spectacles, and Fig. 7 shows the case of an eye with its crystalline lens removed and cataract spectacles placed in front.

Here when the glass is sufficiently in front of the eye to clear the eyelashes, the back nodal plane is brought forward in front of its normal position, and the size of the image on the retina is larger than would be the case with the normal eye. In the lowest figure it is more than half as large again as it was before the crystalline lens was removed, although the focus is still very nearly on the plane of the retina.

The subject also becomes of importance in considering the case of astigmatic lenses used to correct abnormal corneal curvature. In this case the image of lines in one meridian may have been brought by a cylindrical spectacle lens to a focus identical with that of the lines in the other meridian ; but the two images may not be identical in size, as the nodal planes corresponding to the two meridians are probably not the same. A further interesting question in this connection is the use of the new Toric lenses. The use of these lenses would certainly have somewhat more effect upon the displacement of the nodal planes than an ordinary lens, owing to their meniscus shape, and it would appear to be a question that should be worked out so that if possible their peculiar properties in this respect should be made use of. It will no doubt be found that it is only in cases where high-power lenses are employed that the question becomes of importance, but it is just these cases that are so difficult to correct exactly with lenses.

Time will not permit me to pursue the question further in this paper ; my object has been to call attention to this method of studying the subject, as I feel sure that it will well repay those who may take the matter up and carry the investigations further.

Dr R. M. WALMSLEY in opening the discussion bore testimony to the great value of the paper, which he desired to think over, as he had not read it beforehand. Dr Walmsley commended to all those who use English text-books these methods, which were so very fruitful, not only in solving the problems before us, but in suggestions for future work.

Mr HORACE BECK (*communicated*) was very glad this paper had been read, as he thought that the positions of the principal planes of an instrument were not sufficiently considered when many instruments were originally designed. Their position not only affected the optical construction, but in many cases the mechanical construction of the mounts. The telephoto lens was a good example of this. In such lenses the principal planes were frequently 20″, 30″, or even more in front of the front of the camera. If the lens is deflected through a small angle, due to the attachment to the tripod not being rigid, or the front of the camera shifting from vibration, the motion of the image on the plate was as great as the motion of a point attached to the lens, and projecting 20″ or 30″ in front of it. Thus an angular motion of 1° on the lens would move the image more than half an inch on the plate. If this were more fully realized, people would be more careful in selecting suitable cameras for telephoto work.

Dr C. V. DRYSDALE desired to say that he had found this particular method of dealing with optical instrument problems of great value recently in telescopic work. Taking, for example, the ordinary Galilean glass, one of the principal points determined was the magnitude of the size of the field ; and the Galilean glass happened to be one in which we had a small field of view, simply because the equivalent planes of the system were beyond the range of the eye. They were in a position which the eye could not actually reach. As Mr Beck had shown, by making the lens of different shapes we might put the equivalent planes almost where we pleased. Dr Drysdale had taken considerable interest lately in trying whether the field of view of some of these glasses could not be materially increased by pushing forward the principal planes, by lenses or otherwise, and he believed that there were great possibilities in the method. He was very much interested in what Mr Beck had said about the effect of spectacle lenses on the eye, because it was interesting to note that

he had exploded the fallacies which had existed as to the magnification when there were lenses in front of a myopic eye.

Mr G. St L. Carson had felt for some time that, especially in England, practical opticians are turning their attention too much to a discussion of aberrations. He thought that all the possibilities of the Gauss theory had not been thoroughly worked out, and that this was Mr Beck's opinion also. On that account he was particularly glad that the paper had been read, and that it had come so early in the programme.

Professor Sylvanus P. Thompson.—"I consider the paper a very valuable contribution to this part of the science of optics, because it goes a little beyond the ordinary effects of the Gauss theory, applying it in a thoroughly useful way to the combination of lenses as they occur in optical instruments, both telescopes, microscopes, and projectors. So far, it is of very great service. Four or five years ago I wanted to see whether one could not make use of the conception of the Gauss planes for the purpose of systems having widely separated lenses, and I drew out several diagrams of particular combinations; for instance, the very simple case of two lenses of equal power but of opposite sign. Take any case, such as plus 4 dioptrics and minus 4 dioptrics—a plano-convex lens, and a plano-concave one which fit together and make simply a flat piece of glass. One could hardly say exactly what its properties were as an optical system; it has very many. Slowly separating the lenses away from one another, one passes through the stage of the lenses having definite powers until they get to an infinite distance away. If one tries to draw out a diagram of the equivalent planes, the focal planes and Gauss planes, at all the different stages, one after another, and plots out the positions in curves, one gets a picture of what that particular system will do. Take any other combination—two positive lenses, say of unequal power, placed in contact, and then separated gradually from one another to see what becomes of the focal planes and principal planes. Mr Beck has attacked the problem in a simple and most delightful way. I plotted out curves showing how the different planes shifted one from another, and I found it most instructive. I gave some of them to my students, but I never published them. I am happy to see that Dr Drysdale has recently published just the same thing in a very complete form in the papers he has been giving in monthly instalments to the world, and I sincerely hope that he will also publish those papers, because they will be of great value to students of optics. One word as to why it is that we in England have made so little use of the theory of Gauss, which was given to the world something like seventy years ago. Why is it? I believe it is because the Science and Art Department never allowed us to study the subject of optics decently. It mixed it up with Heat and Sound and other things until I made a fuss about it, and it would give no credit to a student who was studying optics unless he spoiled the concentration of his studies by mixing them up with heat and sound. The day for that kind of thing, I hope, is past. I sincerely wish we could bring back the days when so many thousands of students up and down the country in evening classes were going in for examinations at the Science and Art Department, only I should hope that they would no longer go in for those mixed examinations, but that they should be allowed to concentrate their studies on the real study of optics. The number of optical students now, I believe, is very small compared with the number twenty years ago which were studying the hybrid subject of heat, sound and light; and I cannot help feeling that the teaching has been very largely perverted and diverted from its real course by the absurd rule of the Science and Art Department."

Mr S. D. Chalmers.—The method of considering the microscope as a complete system gave very valuable results. It might be employed, for example, to measure the NA of a microscope objective as it was actually used.

"The equivalent focal length of the microscope $= \dfrac{250}{m}$ where m is the linear magnification as used.

But it also $= \dfrac{h}{n \sin u}$ where h is the radius of the Ramsden circle, and $n \sin u$ is the NA.

Thus we have $NA = \dfrac{h \times m}{250}$, and it is only necessary to measure the radius of the Ramsden circle and the magnification to determine both the latter and the NA actually used."

The PRESIDENT desired, in conveying the thanks of the Committee to Mr Beck for his paper, to emphasise his opinion of its importance. It was a very great thing that, in this opening paper of our Convention, we should have attention directed to the philosophic study of the optical instrument as a whole, and not merely to the individual details of individual instruments. This method of considering telescopes, microscopes, and other instruments in a combined manner, by a consideration of the equivalent planes which have properties common to all instruments, would, he thought, do much to help forward our own knowledge of the instruments, and our own study of the subject.

DISCUSSION ON ABERRATIONS.

Introduced by Dr C. V. DRYSDALE and S. D. CHALMERS, M.A.

THE SPECIFICATION AND MEASUREMENT OF OPTICAL ABERRATIONS.

By Dr C. V. DRYSDALE.

AMONG the subjects which it is desirable to have discussed before this Congress with a view to arriving at some consensus of opinion, perhaps none are so important as the defining of the meaning of the various optical aberrations, and the standardising of methods of testing and comparing the performance of different instruments.

The importance of this discussion is threefold. In the first place, it may be of important assistance to the designers and manufacturers of optical instruments, who at present are somewhat at variance on some of the criteria leading to design. In the next place, the possibility of having the various aberrations numerically stated would enable the users of such instruments to select them with reference to their special requirements. It is a well-known fact, that in the higher class of optical instruments, extreme perfection of one or more attributes of the instrument is frequently attained by the sacrifice of perfection in others ; and in the absence of exact information, an individual frequently obtains an instrument which, though of the greatest excellence in several respects, may be deficient in the most important feature for which he requires it. As instances may be quoted, the employment of microscope objectives of very high NA for work where depth of focus is required, and the use of photographic lenses working to apertures of F 4 for process work, in which the zonal spherical aberration when used for apertures of F 22, as is common in process work, may be larger than with a cheap lens of small aperture. Lastly, and most important, the adoption of some definite tests and the certification of optical instruments would deal the death-blow to the worst feature of the optical industry, namely the continued misrepresentation, both unintentional and otherwise, as to the quality of the various instruments. It is an open secret that at the present time there are no official tests or certificates which will discriminate between the performance of the better types of instruments; and while this remains the case, it is obviously impossible to expect manufacturers to refrain from claiming the greatest perfection, or that much encouragement will be given to the attainment of it. The adoption of a satisfactory method of defining and measuring the aberrations of optical instruments will be the most important step towards raising the status of the optical industry in the country.

In dealing with the general principles of aberrations in optical instruments, it must not be forgotten at the outset that optical image-forming instruments fall naturally into two distinct classes—objective and

subjective. To the former class belong all instruments in which an image is formed upon a screen, such as photographic lenses, projection apparatus, etc., while the latter includes instruments such as the telescope and microscope, in which the image is virtual and is only visible to a single observer. In the former case the instrument is complete in itself, and its only function is to produce a perfect image upon the screen, but in subjective instruments the final object is to produce a perfect image upon the retina of the observer, and this depends upon the visual apparatus as well as upon the instrument. Instruments for subjective use therefore should have their aberrations defined with respect to the normal eye, and the tests made on them should either be visual in character, or preferably objective and corrected for the errors of the normal eye.

Next, aberrations should be specified absolutely and relatively, the former with the object of stating the actual performance of an instrument; the second in order that the comparative merits of various forms can be arrived at. The basis of this comparison needs settling.

Lastly, aberrations may be classified into primary, secondary, tertiary, etc., depending upon the degree of approximation. This distinction is mainly of importance to the designer and manufacturer.

It is evident that a great many complex questions are involved, and it is hoped that discussion following this paper will settle them.

We have therefore made the following classification of aberrations into—

     A. Objective or Subjective.
     B. Absolute and Relative.
     C. Primary, Secondary, or Higher.

and it is proposed to very briefly discuss this classification.

Aberrations of Objective Systems.—At the outset it seems necessary to come to some decision as to the definition of an aberration. Aberrations have been specified as the distance between the foci of two sets of rays or longitudinal aberrations. This is notably the case with radial astigmatism, where the distance between the primary and secondary focal lines is usually taken as a measure of the defect. In other cases, the diameter of the circle or patch of least confusion is regarded as the criterion. Neither of these are perhaps the most suitable method of expression. The ultimate object of an optical system is to give an image of as perfect a character as possible on a surface that is usually a plane. What we are therefore concerned with is simply the lateral departure of the edges of a pencil from the point in a plane to which they should be brought in a perfect optical system.

It only remains to select the plane and define the perfect optical system. Now the simple approximate formula of Gauss would, if true for large apertures and angles of view, satisfy the conditions for perfectly sharp images in collinear relationship, and would at the same time specify the image plane. We may therefore define the perfect optical system as one in which all the light emerging from one point of the object plane is focussed to a single point on the corresponding Gauss image plane and in the exact position given by the Gauss formulæ. Any departure of the light from this position constitutes what should be termed the *lateral aberration* or *absolute aberration* of the pencil, while this lateral departure divided by the distance of the image plane from the principal plane of emergence may be termed the *angular aberration* of the pencil. These definitions are consistent with the all-important mathematical theory of Von Seidel, and may be said to be the fundamental assumptions upon which the whole of his work is based.

The primary aberration, or aberrations of the first order are of seven well-known kinds:—

     (*a*) Chromatic aberration of the image plane.
     (*b*) Chromatic aberration of the magnification.
     (*c*) Central spherical aberration.
     (*d*) Coma.
     (*e*) Radial astigmatism.
     (*f*) Curvature of the field.
     (*g*) Distortion.

Definitions of the Various Aberrations.—If the general definition of an aberration above stated be accepted, little need be said about the separate errors.

Central Chromatic Aberration.—In this case there can be little doubt that, so far as instruments intended for visual observation are concerned, whether objective or subjective, the standard plane should be that for the D line, and the chromatic aberration of the image plane should then be the diameter of the disc formed on the axis at full aperture for either the C or F line, whichever is larger. For photographic instruments the standard plane might still be for the D line, and the size of the disc for the F or G line might then express the chromatic error.

Chromatic Differences of Magnification.—Here the amount of the error would similarly be the distance of the focus of a narrow pencil passing through the centre of the diaphragm for the C F or G line respectively from that for the D line on the plane at some specified angle of view. This angle would probably be the extreme nominal covering power of the system when this is known.

Spherical Aberration would be on this basis the diameter of the disc produced at full aperture on the axis on a plane focussed for a narrow central pencil using sodium light.

Coma might be defined as the difference between the distances of the top and bottom edges of the patch formed by an oblique pencil at the specified angle, and at full aperture from the position on the plane to which the centre of the incident pencil is refracted.

Radial astigmatism would similarly be expressed by the difference between the diameters of the elliptic patch formed on the standard plane at full aperture and at standard obliquity, or their sum if the primary and secondary foci lie on opposite sides of the standard plane.

Curvature of the field might be measured by the mean diameter of the patch, or the half difference of the diameters when the focal lines fall on opposite sides of the screen at full aperture and standard obliquity.

Distortion on the basis of these considerations would be measured by the displacement of the image for a narrow pencil at the standard obliquity intersecting the axis at the centre of the diaphragm, from the position given by the Gauss relations. If x_2 is the lateral distance of the image from the axis, x_1 that of the object, then $x_2 - mx_1$ will be the distortion, m being the Gauss magnification $\frac{v}{u}$.

Subjective Aberrations.—In dealing with aberrations from the subjective point of view it is necessary to first decide upon an important point. We may either consider the eye as fixed, in which case the function of the instrument is to produce a perfect image over the whole of the retina; or as rotating, which implies that both central and oblique pencils from the optical system should be of the same character, such as to be focussed sharply upon the macula when the optic axis is coincident with the axis of the pencil. Of these two it would appear that the second is the more rational, as, when the eye is fixed, all but the one point is seen by indirect vision; in which case small errors of definition are not of consequence, while, if an object is intentionally viewed obliquely, the axis of the eye is rotated into that direction. We may therefore take the view that for an optical instrument to be perfect for subjective use, both oblique and central pencils should be of exactly the same character, and have equal and opposite chromatic and spherical errors to those of the normal eye.

So far as the writer is aware, no determinations of the amounts of the spherical and chromatic defects of the eye have been published, and it seems advisable that investigation should be made on these points as, if definite figures were available, designers could use them in their calculations. No great difficulty should be experienced in determining the amount of chromatic aberration. The writer has found it convenient to employ a pair of small apertures which can be placed in front of the eye and separated until a distant point of light can only just be seen through both. When this is the case the two apertures are at the extreme edges of the pupil, and the point of light appears coloured. By interposing a prism of suitable angle in the course of the light to one of the apertures the colouration may be compensated. The writer has used the Risley rotating double prisms for this purpose, and from rough measurements on his own eyes is inclined to think the amount of chromatic aberration in

the eye between the C and F lines amounts to about ·7 dioptre, or approximately the same as for an uncorrected lens of the same focal length as that of the eye, as would be expected.

The determination of the spherical aberration appears to be much more difficult owing to its small amount. Perhaps the most convenient method would be to use a microscope with a small source of monochromatic light and a correction collar objective, or any other instrument into which a variable amount of spherical aberration can be introduced. By adjusting the objective until the spherical aberration was apparently perfectly eliminated according to the eye, and objectively testing the microscope so adjusted, the amount of the error might be found. It appears essential that any tests of the errors should be made on normal living eyes.

Measurement of Objective Aberrations.—There are three principal methods by which the amount of the aberrations of an objective system may be measured. First, we may actually examine the image formed in the Gauss image plane by a microscope or eye-piece in the manner employed by Mr Conrad Beck in his apparatus for testing photographic lenses, and directly measure the dimensions of the patch of light so formed. In the second place, we may actually find the foci for different colours and for various portions, zones, or meridians of the system, and thereby deduce the amount of the various defects. This method has been followed by most of those who have worked at this subject up to the present, and forms the basis of the tests made at Kew and by Moessard with his tourniquet. Thirdly, we may deduce the amounts of the aberrations from observations taken considerably inside and outside the focus, the beam of light passing through the system being preferably split up into a number of narrow pencils by means of a perforated disc interposed before the system. This is the basis of the Hartmann test.

The writer's experience does not allow him to speak with any degree of certainty as to the relative merits of these various methods of procedure. Were it not for diffraction troubles, the preference should lie with the first method, as a high degree of magnification of the disc could be obtained and the amount of the defects directly determined with the aid of a perforated diaphragm, as in the Hartmann test. The use of such a diaphragm might also do much to eliminate diffraction troubles, as the centre of the diffraction system formed by a narrow partial pencil would indicate the true axis of that pencil. Should this not be practicable, the Hartmann test is to be preferred to the second method as diffraction troubles are always greatest at the focus. The Hartmann test is practically unaffected by diffraction, and it has the further advantage of requiring fewer observations on the system itself, although the after measurement may be somewhat lengthy. If this method could be used equally well visually instead of photographically, it would be an advantage, and there seems no great difficulty about this.

Before dismissing the subject of the measurement of objective aberrations the writer would like to call attention to the advantage of employing reflection or auto-collimation as an aid to the other tests. If by means of a plane or spherical mirror the light is made to traverse the system twice by approximately the same path, its aberrations may be doubled in magnitude and thereby be more easily estimated. If the optical system is mounted axially, and the mirror is normal to the axis, the central chromatic and spherical aberration will each be doubled. Coma will be eliminated owing to the symmetry of the equivalent optical system about the mirror. By using a microscope or telescope with a plane unsilvered mirror in its interior, both as the luminous origin and the testing arrangement, great convenience and accuracy can be secured.

It will not be necessary to deal with the measurement of the aberrations of subjective instruments separately, as such instruments should be tested in the same way as objective instruments, and the correction for the normal eye applied to the results. The only essential difference in treatment is due to the fact that in subjective instruments the emergent beam is approximately parallel, and cannot therefore be directly focussed on a screen. By the employment of an auxiliary convergent lens of low power, or of which the aberrations are already known, the aberrations of the instrument should be readily determinable.

Relative or Comparative Aberrations.—Coming now to the expression of the results in a comparative form, the mathematical investigation of the aberrations of the first order leads to the following :—

The angular central chromatic aberration (the size of the chromatic disc on the standard plane divided by the second conjugate distance) is proportional to the relative aperture of the system (effective aperture divided by equivalent focal length).

The angular oblique chromatic aberration (distance between the images for lights of standard colours divided by the second conjugate distance) is proportional to the angle of obliquity.

The angular spherical aberration (of the first order) is proportional to the cube of the relative aperture.

The angular coma is proportional to the square of the relative aperture, and to the obliquity.

The angular astigmatism is proportional to the relative aperture and to the square of the obliquity.

The angular size of the disc produced by curvature of the field is also proportional to the relative aperture and the square of the obliquity.

The angular distortion is proportional to the cube of the obliquity.

As in the mathematical investigations of these aberrations, angles and their tangents are convertible, and the tangent of the angle of obliquity is immediately obtainable by dividing the lateral distance of the image from the axis by the emergent conjugate distance, it would appear most convenient to adopt the following definitions :—

Relative Central Chromatic Aberration.—This would be the angular central chromatic aberration, divided by the relative aperture. Or in symbols, $C_c = \dfrac{\Delta x}{v} \dfrac{f}{d}$ where Δx is the diameter of the chromatic disc, v the emergent conjugate distance, f the equivalent focal length, and d the effective aperture of the stop. For microscopic objectives the numerical aperture would be substituted for $\dfrac{d}{f}$

Relative Oblique Chromatic Aberration.—Following the above lines, this would be obtained by dividing the angular oblique chromatic aberration by the tangent of the angle of obliquity. This is obviously, however, the same as $\dfrac{\Delta m}{m}$ or $\dfrac{\Delta x}{x}$ where x is the lateral distance of the point from the axis, and Δx its chromatic displacement.

Relative Spherical Aberration.—This will simply be the angular spherical aberration divided by the cube of the relative or numerical aperture, or $\dfrac{\Delta x}{v} \dfrac{f^3}{d^3}$.

Relative Coma.—In the same way the relative coma will be expressed by dividing the angular coma by the square of the relative or numerical aperture, and by the tangent of the angle of obliquity, or $\dfrac{\Delta x}{x} \dfrac{f^2}{d^2}$.

Relative Radial Astigmatism.—This will be the angular astigmatism divided by the relative or numerical aperture and the square of the tangent of the angle of obliquity, or $\dfrac{v\Delta x}{x^2} \dfrac{f}{d}$.

Relative Curvature.—From the definitions above given we have relative curvature as the angular curvature divided by the relative or numerical aperture and the square of the tangent of the angle of obliquity as with astigmatism, or $\dfrac{v\Delta x}{x^2} \dfrac{f}{d}$. But on working out we find that this expression becomes $\dfrac{f}{2r}$ where r is the radius of curvature of the field containing the circles of least confusion. The relative curvature therefore on this basis simply depends upon the ratio of the absolute radius of curvature to the equivalent focal length as would be the natural way of expressing it.

Relative Distortion.—Finally, relative distortion will be expressed by dividing the angular distortion by the cube of the angle of obliquity, or $\dfrac{\Delta x}{v} \dfrac{v^3}{x^3}$, which may be written $\dfrac{\Delta x}{x} \dfrac{v^2}{x^2}$ or $\dfrac{\Delta m}{m} \dfrac{v^2}{x^2}$ or $\dfrac{\Delta x}{x \, tan^2\theta}$.

In the above we might have taken the angle of obliquity in degrees, but it would obviously have given us extremely small values for the relative errors.

Aberrations of Higher Orders.—It is not proposed to deal here to any extent with the subject of the higher aberrations, as they are so numerous and their relations are so complicated. A few words will however be devoted to one or two of them, in so far as they affect any of the conclusions above stated.

Dealing first with chromatic aberration, the definitions above given would not require modification for secondary or tertiary spectra. Chromatic differences of spherical aberration could be dealt with by stating the amounts of spherical aberration for two or more monochromatic lights.

Spherical aberration requires more consideration. The higher orders of this defect, or even the "spherical aberration" of the first order for figured lenses, will not depend on the cube of the relative aperture as above stated. In fact, the spherical aberration may be absolutely corrected for full aperture or any particular zone, and be of fairly considerable amount at lower aperture. Should this be found in testing, the best course would probably be to state the values on precisely the same basis as before for two or more values of the aperture.

The oblique errors of higher orders might perhaps be similarly expressed for one or two different apertures, and for, say, two values of the obliquity, the full covering power and half that angle.

It appears to the writer, however, that such great advantages would accrue from merely settling the tests and expressions for the elementary aberrations, that it would be better for the higher aberrations to be left on one side for the present.

Conclusion.—In concluding this purely suggestive paper, the writer would call attention to the proposition made by Mr Ryland at one of the recent meetings of the Optical Society, that a scale of marks should be adopted for optical instruments similar to that applied to watches at the Kew Observatory. Of course much will have to be done in the direction of making a satisfactory scheme of tests before this would be possible, but in view of the impetus given to accurate watch-making by the Kew certification, the proposition seems worthy of being kept in sight, and acted on as soon as it may be found practicable. The summary of the properties of the lens might perhaps be on a schedule stating the equivalent focal length of the system, the positions of its principal or nodal points, and its magnification if a subjective instrument. There should also be stated the maximum numerical or relative aperture, the maximum illumination for visual or actinic light, the maximum field of view and the uniformity of the illumination, with marks for each, while marks should also be given for the perfection of the mounting, freedom from flare-spot, parallelism of axes in binocular instruments, etc. Finally, dealing with the Aberrations, a form of the following kind might be adopted :—

NATURE OF DEFECT.	ABERRATION.		MARKS.	
	Absolute. mm.	Relative.	Maximum.	Awarded.
Central chromatic aberration at full aperture				
Oblique chromatic aberration at specified field				
Central spherical aberration at full aperture				
Coma, at full aperture and specified field				
Radial astigmatism at full aperture and specified field				
Curvature of field at full aperture and specified field				
Distortion at specified field				

The writer regrets that time has not permitted his consulting any authorities on this important subject, and is conscious that many of the suggestions and statements made above are open to criticism. He hopes, however, that they may form the basis of a useful discussion.

ABERRATIONS.

By S. D. CHALMERS, M.A., Head of Department of Technical Optics, Northampton Institute.

PROGRESS in the design of optical systems is dependent to a very large extent on the exact specification of the aberrations of the system, whether these aberrations be obtained from the theoretical design or the actual performance of the system.

It is of the utmost importance that the numerical specifications in these two cases should be directly and strictly comparable, and the methods employed should be of sufficient generality to permit of a thorough test of the system. In addition it is desirable that these results should be capable of expression in a form which gives a definite numerical value to the definition at any point : the numerical measure of the aberrations should be sufficiently definite and detailed to allow the excellence of definition to be determined from the calculation of the aberrations only.

The only method of calculation universally applicable is the trigonometrical method ; this permits the ready calculation of the path of any ray through the system, though the calculation of rays not in one plane involves considerable labour. The results enable us to express the points in which the ray intersects the focal plane and any plane parallel to it. In this way the positions of the intersections of various rays proceeding from the same object point may be determined, and one of these rays being taken as chief ray, will give the distortion expressed as a lateral aberration ; while the definition at this point is indicated by the lateral aberration of the other rays relative to the chief ray.[1]

It will be noticed that the calculations also give directly the definition at any plane parallel to the focal plane. Various rays have been calculated through the lens $R_1 = +100$, $d_2 = 6$, $R_3 = -100$, $n_D = 1.5168$, the object being taken on the axis. The rays through three points on the lens are considered, on the axis, at a distance of 10 mm. from the axis, and at a distance of 5 mm.
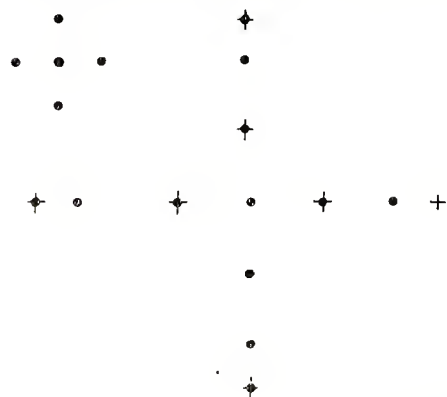
The intersections of these rays with the focal plane are drawn out on a scale of 50 : 1. Fig. 1 represents the intersections of a ray with the focal plane. If any other plane be chosen as focus, the intersections can be immediately expressed. It is, however, convenient to indicate in this diagram the aperture to which the intersection corresponds.

The aperture of the lens is drawn out on any convenient scale, and the lateral aberration of each ray relative to the chief ray is measured from the corresponding point on the aperture diagram (Fig. 1). In this figure the aperture diagram is indicated by ●, and the actual rays by +, thus the distance between ● and the + represents the lateral aberrations on the scale shown.

Scale = 1 m/m

Fig. 1.

Measurement of Aberrations.—Our standard method of testing the actual system should give the same information as the trigonometrical method of calculation. It should be absolutely universal and exact, and allow of a subsequent choice of the best focal plane (which may be other than the theoretical Gauss

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[1] Formulæ for this purpose are given, Steinheil u. Voit, *Angewandte Optik*, p. 238 ; and Everett, *Proceedings, Physical Society*.

plane or the plane which gives the best central definition). The method devised by Professor Hartmann[1] gives this information (although it has not yet been applied to microscopic objectives) for as many rays as may be desired.

Various beams of light, proceeding from a small circular source are isolated by means of a diaphragm with a number of small circular apertures (Fig. 2); these beams converge to form on the focal plane the image of the source of light, but may be intercepted on two planes at equal distances inside and outside the focal plane. If these distances are suitably chosen, the centre of the section of each beam can be determined, and thus the direction and position of the ray from the centre of each aperture is obtained; thus we have the point of intersection on the focal plane and on any parallel plane. The sections of the beam are most frequently obtained by exposing a photographic plate at the desired positions, and the final values of the lateral aberrations are obtained exactly in the same form as by trigonometrical calculation.
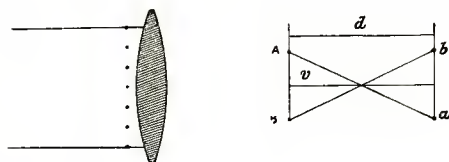
Fig. 2.

The method is susceptible of very great accuracy, and can be used to test how accurately the design has been carried out, the same rays being tested as were originally calculated. In addition, other rays can be tested, thus avoiding much of the labour involved in calculating the oblique rays of a system, especially those not in one plane. It is specially applicable to telescopic and photographic, including spectroscopic, apparatus.

The methods of measurement are, however, in their original form, much too laborious except for large astronomical refractors, and I have in practice found considerable simplification possible when dealing with photographic lenses. To make the results readily expressible in terms of the usual aberration specifications, the diaphragm is placed at the plane of the stop. The principle of rotating the lens about the back nodal point enables the lens to be tested at various angles, a fixed collimator or distant source of light being used; a series of photographs inside focus can be taken on one plate and a corresponding series on another provided the lens can be set at definite angles. For this purpose the Beck lens-testing bench is used, a special fitting being attached to permit of the plate swinging with the lens holder, thus allowing all exposures being made on plates which are kept parallel to the focal plane. The photographs are examined microscopically, and a camera lucida used to draw a diagram showing the centre of each of the spots; when the first diagram is complete the other photograph is placed on the stage of the microscope, being reversed to bring corresponding spots in corresponding positions and compared with the other.

Where the visual best focus corresponds exactly with the best photographic focus, the two diagrams should be as nearly as possible coincident; but the magnification can be altered (within small limits) till the two central diagrams are as nearly as possible coincident; the whole series of diagrams are then drawn off, care being taken to bring each central spot into exact coincidence with the corresponding spot of the other diagram.

The distance between the two corresponding spots will then represent the lateral aberrations of the ray, on the scale of $m_1 + m_2 : 1$ where m_1 and m_2 are the two camera lucida magnifications.

To permit of the focus being modified subsequently, it is desirable to redraw the second diagram with a different magnification m_3. This corresponds to a change in the position of the focus.

$$= \text{distances between exposures} \times \frac{m_3 - m_2}{m_1 + m_2}.$$

In drawing diagram—● indicates inside focus.

+ indicates outside focus, magnification m_2.

o indicates outside focus, magnification m_3.

[1] Zeitschrift für Instrumentenkunde, 1904, translated *The Optician*, 1904.

Fig. 3 represents the photographs with the single lens calculated above, the separation between the photographs being 2 cm.
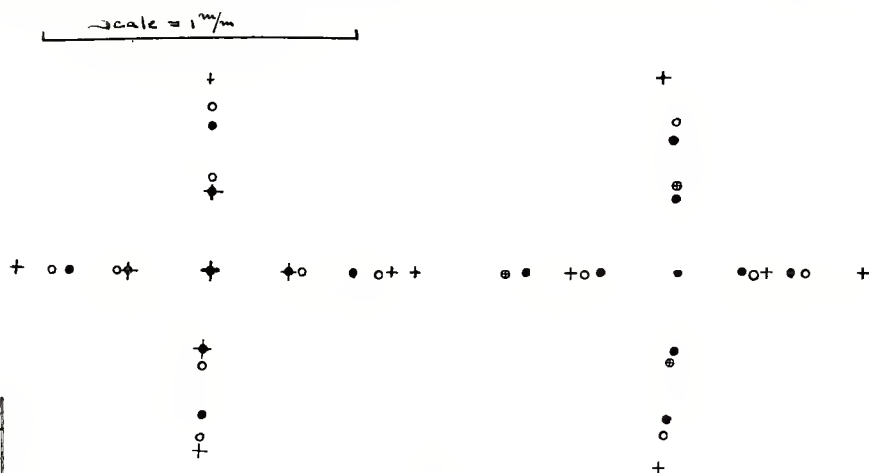
Fig. 3.

The photographs are taken on the axis and at an angle of 5 deg., and it is specially interesting to compare the first with the corresponding diagram of Fig. 1.

The results would be more nearly in accord if the photographs were taken with the monochromatic light used in the calculations. But the results are sufficient to show the possibility of checking the calculations for the case of one positive lens.

In the case of telescope objectives in which the crown is in front, it would be possible to check the calculated rays through the system. This is also possible whenever the lens system remains positive when any number of the back lenses are removed; thus in a double symmetrical like the Unofocal photographic lens, the results at the end of the first lens, the first combination, and the whole system could be obtained to compare with the trigonometrical results.

Fig. 4 shows the results of a test on a modern anastigmat of 150 mm. focal length. It is interesting to note the behaviour of the rays which pass out of the one plane, these corresponding to the apertures on vertical lines.

It is thus possible to obtain the aberrations of a system either by calculation or measurement, and to express them in the same diagrammatic form; but it would be exceedingly desirable to deduce from these aberrations a numerical value which should give the definition; and thus the latter could be absolutely predicted from the value of the aberrations, as calculated or measured.

There are two methods of procedure in attempting to set up such a standard; the one, purely empirical, to determine by a series of observations the aberrations which give good definition, and the behaviour of each combination of aberrations; the other method is to determine theoretically the effect of aberrations on some important factor which we know influences the definition, such as the distribution of light intensity, and to determine how far results deduced in this way agree with observation and experiment.
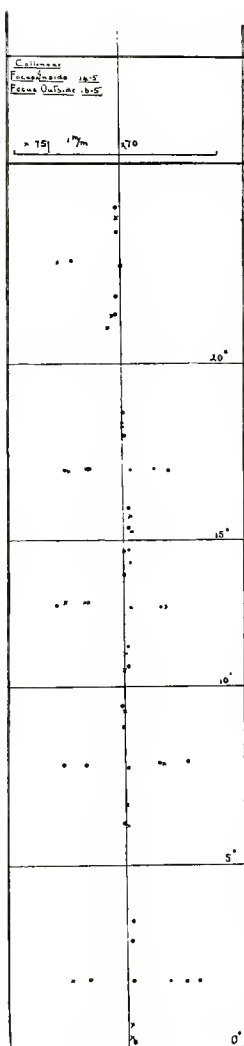
Fig. 4.

An attempt at the solution of this problem has been made by Strehl and others, for the case of central definition for telescope objectives and also for microscope objectives, though in the latter case the impossibility of accurately measuring aberrations has precluded the testing of the results. But the results of the method in the case of telescope objectives have so far confirmed the theory, and it is with the hope that those of you who have the opportunity may make further tests that I give a short description of the method.

In the case of telescope objectives it is easy to determine the brilliance of the central image as compared with its possible value. In Fig. 5 F is the focus of the system, OQ the emergent curved wave, OP the spherical wave of equal curvature at O. The light proceeding from Q to F will traverse a distance QF instead of $OF = PF$,
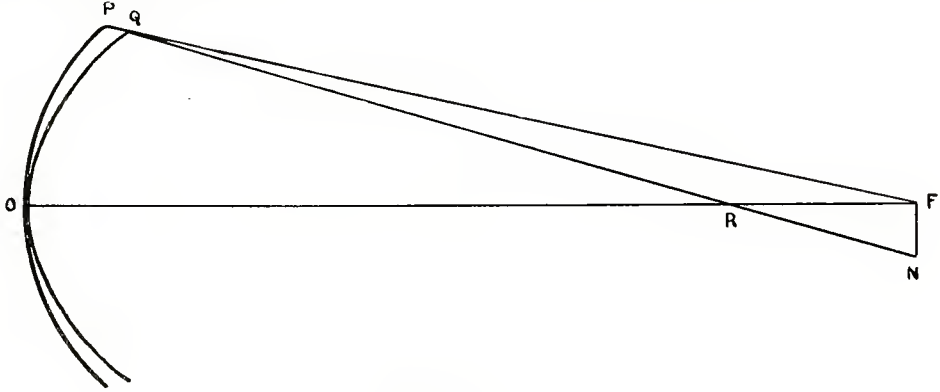
Fig. 5.

And thus there will be a difference of phase at F of $2\pi \dfrac{PQ}{\lambda}$.

The lateral aberration $FN = \dfrac{\sin RQF . QF}{\sin QNF}$,

But $\sin RQF = \sin (\sigma_1'' - \sigma_0)$,

$= \tan \sigma_1 - \tan \sigma_0 \times \cos \sigma_0 \cos \sigma_1$.

Tan $\sigma_1 = \dfrac{d\beta_1}{dx}$, where x is the ordinate of Q, and β_1 is its abscessa.

Similarly tan $\sigma_0 = \dfrac{d\beta_0}{dx}$.

We have $FN = QF \times \dfrac{d\beta_1 - d\beta_0}{dx} \cos \sigma$,

$\therefore \dfrac{d\beta_1 - d\beta_0}{dx} = \dfrac{\text{lateral aberration}}{QF} \dfrac{1}{\cos \sigma}$,

$\beta_1 - \beta_0 = \displaystyle\int_0^x dx \dfrac{\text{lateral aberration}}{f}$

where $f = OF$ and σ is taken small.

Thus the light reaching F from Q has $\displaystyle\int_0^x dx \dfrac{\text{lat. aberr.}}{f}$ difference in path, i.e. $\dfrac{2\pi (\beta_1 - \beta_0)}{\lambda}$ difference of phase.

The intensity at F is proportional to the square root of

$$\left\{ \int_{x=0}^{x=h} \left\{ 2\pi x\,dx . \cos 2\pi \dfrac{\beta_1 - \beta_0}{\lambda} \right\}^2 + \int_{x=0}^{x=h} \left\{ 2\pi x\,dx \sin 2\pi \dfrac{\beta_1 - \beta_0}{\lambda} \right\}^2 \right.$$

Thus from a knowledge of the lateral aberrations of the objective the brilliance at the centre can be determined as a fraction of the possible brilliance, and this is a number associated with the definition of the objective.

It would be very desirable that this theory should be subjected to as severe a test as is practicable, as the results of such tests might furnish us with a means of expressing the definition of the objective as a definite number.

The papers of Strehl give various modifications of the method, enabling it to be applied to test the best position of the focus, and to allow for this element in the final estimate of the brilliance of the image ; the calculations are much simplified by the adoption of certain graphical methods for which I must refer you to the original papers.

Mr G. St Lawrence Carson.—With regard to actual aberrations, there are two ways of looking at the point, which I think are entirely diverse from one another, and are likely to remain so for some time. The first is the actual calculation of aberrations and the expression of them in numerical form. That leads one to a certain series of results. Von Seidel got those results in certain cases. For photographic lenses, with which I am concerned, one has to take practice, for the theory is very complicated—if it is a theory at all at present. Taking Von Seidel's results, one gets these five aberrations, and one can calculate the numerical expression for them with a certain amount of labour. Unfortunately, in my own experience, and the experience of most opticians, when you have this calculated expression, if you proceed to construct the lens you will find that even if the two expressions for aberration of the lens are of identical value, the performance of the lens may be entirely different, owing, of course, to the point to which Mr Chalmers alluded—mainly to the distribution of light over the Gauss plane near the image point. That is to say, if you take the theoretical Gauss image, the actual point, very few of the rays starting from the object will go through it ; you must take a small area to correspond to the object point, and consider that all the rays passing through that small area give you the image. Outside that, of course, there is the scattered patch of light, caused by aberration. That patch of light it is that spoils the definition of the lens, and the question, for practical opticians at any rate, is how is it spoiled. It has always appeared to me that it is spoiled ; in the first place, according as this patch is more or less intense, that is to say, you have a certain percentage of loss of light from the actual source. If that scattered light is concentrated very close to the image the brilliance of the actual image and of the scattered patch are nearly identical. If it is scattered over a large area, the brilliance of the image is very great compared with the patch, and in actually taking a negative the patch is really invisible except in the case of a long exposure. Roughly speaking, if the brilliancy of the actual image is about seven times as great as that of the aberration patch caused by all the aberrations, then I think you may say that the lens will give good definition as far as that goes. But there is a further point connected with aberrations which is very much more difficult to calculate, in fact, so far as I am concerned, it has proved practically impossible to calculate, and that is to get a very sharp discontinuity of illumination between the actual image small area and the aberration patch surrounding it. I have myself constructed a lens which answered to the first condition. It gave a certain amount of aberration for oblique central rays, in which the residual light which did not fall upon the image area was widely distributed ; the lens answered the condition that the lost rays should be very much diffused, so as to give very weak illumination compared with the small area which formed the actual image. But I constructed this particular lens so that instead of there being a sharp discontinuity from the image to the aberration area there was a gradual falling off, and when you actually examine the image with a high-power glass, or when you take a negative with it, it does not give the definition you expect at first sight. The definition may appear all right, but as soon as you enlarge it even twice, the want of sharpness in the contrast becomes apparent. I believe that is really one root of the difficulty, not only in constructing lenses, but in predicting their performance. I have tried the effect of calculating the total amount of diffused light, and, as nearly as one can, the area over which it is

spread, and taking the average amount of diffusion illumination (assuming the diffused light evenly illuminates the aberration area), but it was very unsatisfactory.

In some of the aberrations, notably coma, the light is concentrated near the image; but the illumination over the average part of the area and discontinuity cannot be calculated. A more accurate calculation or an experimental measurement is necessary. Any test of the numerical value of the performance of a lens must be of value, not so much perhaps to the optician as to the man who is going to buy the lens; you must give him the assurance that the performance of the lens will give him good definition. Even if you get numerical tests which measure aberrations, it is quite possible that the definition of the lens may be different in two different cases, and until we get round that difficulty, if we ever can, I do not think numerical measurements of aberrations will be of much use from the point of view of the ultimate user of the optical system, although it may be of great use to the optician who is making them. If anything can be done to either devise a practical method of choosing the image area on the Gauss plane, and then obtaining a discontinuity in the illumination of it, and measuring it in practice, I believe that the problem of constructing and testing objectives will be practically solved; but personally, I do not see at present that there is very much hope of doing it. It is not only that the theory is difficult, the practice also is difficult. Although I may take a somewhat pessimistic attitude, I think the initiation of this discussion by Dr Drysdale and Mr Chalmers has been of the greatest value, not only to opticians, but also, I hope, to the users of optical instruments.

Professor SILVANUS THOMPSON.—It is quite clear that the trigonometrical method of following out the individual rays will be more likely to lead to useful results, in yielding measurable quantities which can be specified, than any of the other methods of treating aberrations. As Mr Carson suggests, it is absolutely necessary in dealing with different aberrations to consider the intensity of the light that goes in the different directions. There may, so to speak, be different kinds of aberration in different directions and at different angles, not all equally important in destroying the definition. Dr Drysdale has not suggested among the tests for aberration some other tests which, I think, would be quite equally useful to the users of lenses. For example, in testing as to the absorbing properties of the lens, we may imagine two lenses which are quite equal in other respects, in which the glass is so composed that one will absorb a much larger portion of ultra-violet rays than the other; and they would not be equally satisfactory from the user's point of view, even though their aberrations, strictly so-called, might be exactly alike. With regard to the remark of Dr Drysdale, that the eye, when used in combination with other instruments, cannot be treated always as a portion of a centered system, I suggest that it might be useful in certain instruments to arrange the eye-pieces on them so that they could be swivelled round in some way, that you should be able to de-centre the eye-piece so that it might be used, with all the goodness of its central performance, along some oblique direction to receive oblique rays. I am afraid the suggestion is very impracticable from the makers' point of view, but I do not think it is altogether unworthy of consideration. There are a number of cases—they occur occasionally in an isolated form—where some contrivance out of the ordinary run is worth having. It might also be useful to have an eye-piece made with a fine adjustment, a kind of collar correction, to alter the distance between the field-lens and the eye-lens, and change the aberrations of the eye-piece for any special piece of work. These suggestions may have some value for individual cases.

Dr BURTON.—Professor Thompson's suggestion of a small swivelling movement for an eye-piece is perfectly practicable in certain cases, giving considerable movement. The eye-piece which I have in my mind was used for a focussing screen. The focussing screen was a transparent one, with scratches upon it, so that the eye-piece formed part of one system with an enlarging photographic lens. The instrument was for enlarging stellar spectrograms, and it was necessary to run from end to end of the focussing screen to see that there was good definition in every part. If the eye-piece was merely made to slide laterally, it received no light whatever in the out-lying parts of the field; it was so constructed

that it could swivel about an axis lying in the focal plane, so that it was easy to bring any part of the focussing screen into the view of the eye-piece.

Mr D. E. BENSON.—I would like to suggest, more from the purchaser's than from the manufacturer's point of view, that an addition should be made to Dr Drysdale's specification, at any rate in regard to photographic lenses, and that is that he should give us some idea of the amount of flare the lens possesses—I do not mean the flare that arises from what might be called coma, but the flare that arises from the internal reflection in the lens. I have had through my hands lenses which the makers assured me gave absolutely critical definition; but if you get a small bright light on the one side and a small dark shadow on the other, your plates are ruined by the internal reflection.

THE PRESIDENT said that he thought there were yet a great many difficulties to be overcome before the numerical statement of aberrations would be possible. He also thought that the aberrations should be expressed in terms of the actual measurements of the distances between the best foci, especially in specifying astigmatism and curvature of field.

Dr DRYSDALE in reply said that although he was quite aware of the great difficulties that would have to be faced in the measurements, as he had himself stated in his paper, he was hardly convinced that they were insuperable or as difficult as some of the speakers had indicated. However that might be, there could be no doubt of the importance of the question, or that attempts would be made to solve it; and that being the case, it seemed to him of the greatest importance that the goal to be reached should be clearly defined in order to avoid great waste of labour. With reference to the President's remarks he thought that however the measurements might be made, it was most essential that they should be expressed in the manner indicated in his paper. The test would be conducted by scientific workers who would have no difficulty in converting the results to any desired form. It was quite otherwise with the user of the instrument, who most probably would not be a mathematician. To him the simple question was as to whether the aberration patch on his screen or plate was smaller or larger than a certain size, and the results ought to be expressed in this form.

The remarks of Mr Carson, which were in some respects an amplification of the later part of Mr Chalmers' paper, were most interesting and valuable, but he was inclined to think that the cases cited by Mr Carson were somewhat extreme and not of frequent occurrence. The question of the distribution of light in the aberration patch was, however, of undoubted importance, but he thought it would be met by the measurement of the size of the patch for two or three values of the aperture. Beyond, therefore, increasing the amount of labour involved in making the tests, he did not think this question introduced any fundamental modification to his paper in which the matter had really been considered under the heading of "aberrations of higher orders." He would like to slightly amplify the suggestion made in the paper as to the visual use of the Hartmann test in the Gauss plane. By making a small aperture in a strip of metal which could be moved across the diaphragm aperture from one side to the other in either the meridional or the sagittal plane, and following the centre of the diffraction system on the Gauss plane with a microscope with filar micrometer, it might be possible to obtain the size and distribution of light in the patch with comparative rapidity.

The suggestions of Dr Thompson were most interesting, and especially that of having a correction collar adjustment for spherical aberration in eye-pieces. He hoped that this suggestion would be adopted in practice. Mr Benson would find flare spot referred to in the schedule at the end of the paper.

Finally, he would express the hope that more work would be done on this important subject, and would suggest that it was a matter which a Committee of the Optical Convention and the National Physical Laboratory might usefully deal with.

MR CHALMERS in reply said that he appreciated the importance of the light distribution in the

plane of the lens. The latter part of the paper had been intended to deal more particularly with the cases where definition was so good that the size of the disc of light could no longer be regarded as a fair test of the definition. He appreciated the importance of the sharp discontinuity, especially in photographic lenses.

He thought that the practice of defining the aberrations, such as astigmatism and curvature of field by the distance between the best foci, was liable to lead to false results. He had tested a lens in which there were two positions rather widely separated, at which distinct images of vertical lines were formed.

THE MECHANICAL DESIGN OF INSTRUMENTS.

By Walter Rosenhain, B.A., B.C.E.

THE mechanics of instrument design, if at all fully treated, are fit subject for a large treatise rather than for a short paper; but no attempt at full treatment will be made in the present paper. The object of the author is rather to draw the attention of the makers and designers of instruments to certain facts which are in themselves perfectly well known, but whose consistent application in the construction of instruments is only too often and too sadly lacking.

The key-note of the line of thought to be advanced in the present paper is to be found in the view that a well-designed and well-made machine tool is the ideal prototype of a scientific instrument. For, in the last analysis, a machine tool is nothing but a scientific instrument, since its purpose is to carry out some special operation with accuracy and speed. Of course it is not suggested that the instruments of our laboratories should be made of the size and weight of large machine tools, but the analogy is used rather to suggest a corrective to the tendency to extreme lightness and "delicacy" which is the key-note of many of the productions of the instrument-maker. It is of course obvious that in many cases, lightness and delicacy are absolutely essential, and the author is second to none in his admiration of the skill with which instrument makers have met these requirements. His contention, however, is that there is a strong tendency to carry the refinements requisite in special cases into general use, and thus to produce instruments which are often lacking in mechanical strength and rigidity. He would, in fact, go further still, and suggest to those who originate novel forms of instruments, that such requirements of extreme delicacy are a serious drawback to the use of any form of instrument, and therefore the inventor of novel forms should seek to avoid, as far as possible, these conditions which can only be met by cutting away material and sacrificing strength and rigidity.

The various portions of any instrument fall under one or other of the two great divisions of mechanical productions, Structures and Machines. Those parts whose function it is to act as supports, or to resist the forces exerted upon them by other parts, or by the environment, may be regarded as structural, while the more actively functional parts are portions of a machine. It is almost exclusively in the latter class of parts that extreme lightness and delicacy is ever really essential. In the structural parts of an instrument, unless it be intended for portable use, the question of weight is of minor importance, and the fairly obvious contention of the author is that the design should be such as to secure the essential strength and rigidity of support for the working parts, while allowing of convenient access. Unfortunately, in many cases, these considerations are sacrificed, or at all events ignored, in favour of "elegance of appearance." With some instrument-makers, in fact, this craving for elegance leads to a costly display of lacquered brass and similar finery which is, in many cases, objectionable in use. The contention that in a scientific instrument appearance should be absolutely sacrificed to utility seems so obvious as to require no emphasis; but an inspection of the products of many makers will convince those who are obliged to use scientific instruments under adverse conditions that the

contention requires to be strongly enforced. It is difficult to give examples of the kind referred to without invidiously criticising the products of individual makers, but to take an optical example, spectroscopes and other apparatus, carrying large graduated circles and heavy projecting telescopes, are often built upon a tripod base having no support for the instrument above, beyond a central column an inch or two thick. The result is, that the mere weight of the hand resting upon one of the working screws of the instrument throws a serious strain on the central axis, and visibly deflects the telescope and circle. Were it not for the sake of appearance, and the reduction of weight, a broad hollow column supporting the circle at or near its circumference, and taking the weight of the telescopes as directly as possible, might well be used.

The proper and systematic use of correctly-designed beams in all forms of instrument is another
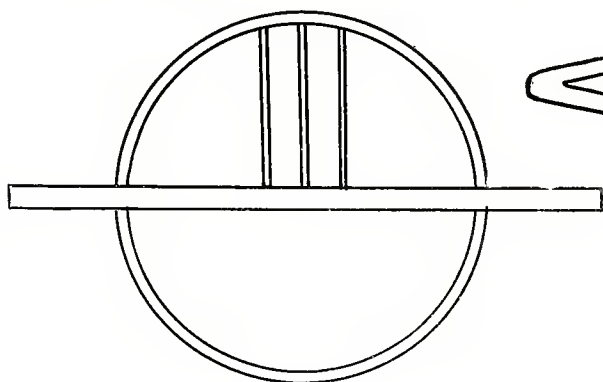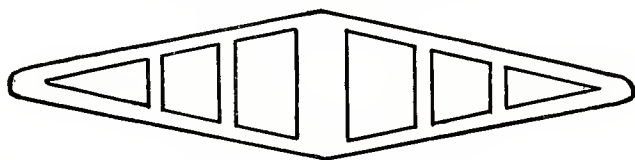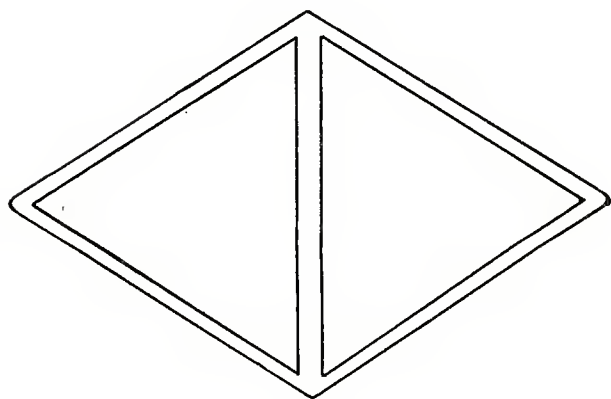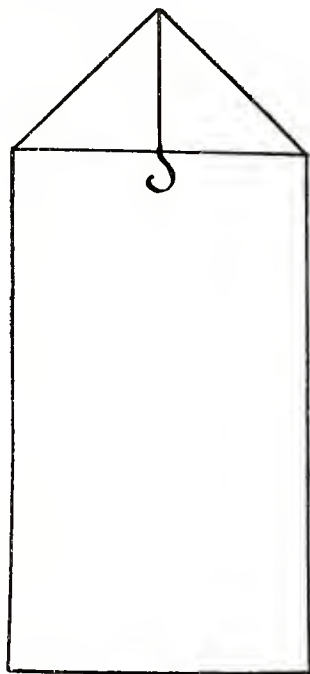
FIG / b

FIG. /a.

FIG. /c.

FIG 2

point frequently neglected. Perhaps the most striking example of this kind may be found on looking through the illustrations in a catalogue of delicate chemical and physical balances. To any one acquainted with the scientific methods of disposing material so as to obtain the maximum of strength and rigidity with the minimum of weight as practised by engineers in the design of roofs and bridges, the principles underlying the design of the majority of balance beams must appear incomprehensible. In place of properly braced beams made up of definite systems of triangulation, such as are to be seen in modern bridges and roofs, we meet with fantastic curved "braces" or systems of unbraced polygons (Figs. 1*a* and 1*b*). As a matter of fact, the problem of the balance beam is so comparatively simple that the proper shape for maximum strength and rigidity with minimum weight can be, and has been, worked out as a problem in maxima and minima, with the conclusion that the ideal shape consists of two equilateral triangles placed back to back, the support being situated in the central rib, and the pans being suspended from the apices of the triangles (Fig. 1*c*).[1] A further illustration of unmechanical design is often to be met with in the frames forming the suspending member of the pans. These are nearly always made of comparatively slender wire, and one would have thought that care would be taken to arrange the material in the most advantageous way. In place, however, of a properly braced frame such as that indicated in Fig. 2, we constantly find frames of curved wire which are of the very shape to give a minimum of strength and rigidity, with the considerable inconvenience in practice that when such a balance is loaded with anything like its nominal maximum charge, the pans and frames are elongated, and adjustment of the pan supports is required.

Turning to an example in a totally different field, we find the structural arrangements of the microscope distinctly unmechanical. The vexed question of the proper form of foot is not to be entered upon here, except, perhaps, to mention that frequently the lateral stability of these instruments is insufficient with the "Continental" form of foot. The point to which attention is to be drawn here is the unscientific arrangement of the microscope tube in respect to its support. This tube, whose delicate and accurate motion is to be secured by the utmost refinement of slow-motion screws, etc., is placed at the end of a cantilever having an overhang of several inches from its support or bearing, with the result that the slight inevitable play of the bearing is magnified many times by the leverage, to say nothing of the strains thrown upon the bearing of the fine adjustment when the coarse adjustment is manipulated, or the tube itself handled or even pressed upon by the face of the observer. And, apart from the regard to conventional appearance, there is no real necessity for the overhang of the microscope tube from its supporting column; at least, an overhang or bridge piece of some kind is in itself required to give a clear space for the stage, etc., but there is no adequate reason why the fine adjustment should not take the form of a hollow screw acting concentrically with the tube of the microscope itself. But even if so radical an alteration be regarded as impossible, the evil of the overhang might be greatly reduced by lessening its amount and placing the bearing of the fine adjustment as near to the axis of the tube as possible. In cases where extreme rigidity is required a further step might be taken by giving the tube another bearing on the opposite side, thus placing the axis of the tube at the centre of a beam instead of at the end of a cantilever.

Turning now to the moving parts of instruments, or rather to the operative, machine-like parts, we find many examples of design that might be improved upon by the strict application of mechanical principles. In this connection the analogy of the machine tool again comes to mind, and the instrument-maker must bear in mind that such a machine must satisfy conditions in many cases more stringent than those imposed on "delicate" instruments, as a very high degree of accuracy must be maintained in spite of constant strain and vibration, and must be maintained under continued use and wear, while, in the case of the majority of instruments, use is only intermittent. Of course it would, for these very reasons, be foolish to incur the expense and inconvenience of great weight and strength where not required, but the ideal of the instrument-maker too often seems to be the watch rather than the machine. Excessively light spindles, flimsy gear-wheels, pulleys inadequate to convey the necessary

[1] Kernot, W. C. *Proc. R. S. Vic.*

C

power, and similar features are too frequently met with ; in other cases mechanical means of attachment by screws, keys, etc., are dispensed with in order to lessen the weight and ostensibly reduce the complication, while in reality certainty of working is sacrificed.

Many of the faults with which instrument-makers and designers are here charged will no doubt be defended on the ground that the features to which objection is taken were so made in order to lessen one or other correction which occurs in the use of the instrument. In a few special cases this contention may be justified ; but the view of the author is that wherever possible a larger correction should be faced, instead of sacrificing the mechanical solidity of the instrument. To the obvious advantages of sound construction is to be added the fact that in many cases this very soundness of construction enables a larger correction to be applied with greater accuracy and certainty than the smaller correction of a differently designed instrument. Friction corrections, for example, will maintain a much more constant and accurately ascertainable value in large, well-proportioned bearings than in flimsy " frictionless " arrangements intended to practically abolish friction but always falling far short of that ideal after any considerable amount of use.

The object of the few criticisms and suggestions advanced in the foregoing remarks has been simply, in the interests of makers and users of instruments alike, to direct attention to the desirability of close adherence to the principles of mechanics in the design of instruments, and in illustration of this view it has been necessary to advance some unfavourable examples of existing practice. It is not, however, in the least the desire of the author to decry the achievements of instrument-makers, to whose constant endeavours the scientific workers of to-day owe their splendid equipment. Nevertheless, criticism, even if its detailed application in practice be more difficult than the author at present imagines, is essential to progress, and it is with progress in view, and in the hope of raising discussion among those best able to deal with such a subject, that the present paper is offered.

Dr Burton considered that Mr Rosenhain had treated the instrument-makers very leniently, having conspicuously refrained from making certain very nasty criticisms. He (Dr Burton) was in essential agreement with the points advanced in the paper, and expressed great respect for substantial engineering work in instruments where rigidity was essential. He (Dr Burton) wished to add some remarks on the question of geometrical design. This subject had been so fully treated in Thomson & Tait's *Natural Philosophy* that it was hardly necessary to draw attention to it, but he considered that the practical importance of such features of design was not as widely recognised as it should be. In practice, where geometric design was used, it was often at the sacrifice of adequate bearing surface ; he (Dr Burton) had even found physicists who were under the impression that geometric design essentially involves *point* bearings, and that separating ordinary contacts, so as to have (for example) five contacts of finite extent was insufficient. This was a great fallacy, and resulted in the design of instruments which were liable to go wrong if they were looked at rather too hard. Taking the engineer's form of box slide, or some other form of slide which could not be made to go wrong except by actual violence, it was only necessary to cut pieces out of the bearing surfaces so as to form five definite contacts, and to keep the slide up to those contacts by means of a spring or a dead weight, to obtain a design which combined the stability and freedom from derangement, as well as the liberal bearing surfaces of the engineer's slide with the advantages of geometric design.

Mr R. S. Whipple agreed with Mr Rosenhain in thinking that the design of the majority of scientific instruments might be improved in some direction or another, but it was always easier to criticise than to help. In many instances an instrument had grown from an original model, which had been made to suit the particular patterns or castings which the maker had in stock when the design was first discussed, and it required a considerable amount of moral courage to put on one side a design which had served its purpose, and re-design *de novo*, unless some very considerable monetary advantage was obvious. It was, further, exceedingly difficult for a maker to forecast the sale of any instrument, and hence, also, to estimate the amount of capital which should be expended on perfecting it.

The large demand for instruments in engineering operations had had, and was still having, a stimulating effect on the instrument-maker. He was beginning to realise—as Mr Rosenhain had put it—that instruments must be capable of standing hard blows without the adjustments suffering, and, at the same time, that the adjustments must be as simple and accessible as possible.

In the old days an instrument-maker was accustomed painfully to build up the various parts of his instruments, whereas now he does not hesitate to use castings wherever possible.

For an instrument to attain the highest efficiency, good design is essential, while good workmanship in the working parts could not be too strongly emphasized. The instrument-maker owed a great debt of gratitude to the inventors and makers of new materials, such as nickel steel, aluminium, phosphor-bronze, and celluloid, for these now played a most important part in modern instrument design. It was to be regretted that in so many cases the instrument-maker had no opportunity of using the instrument he had made, and this made it more than ever important that the user should be in sympathetic touch with the maker.

The PRESIDENT, in asking the meeting to accord a vote of thanks to Mr Rosenhain, expressed his pleasure that Dr Burton had referred to the debt we owe to Lord Kelvin for insisting upon the importance of geometric design in physical instruments ; Fresnel & Huyghens long ago had done good work in that direction. He (the President) found it extremely hard to overcome the instrument-maker's love for brass polish and lacquer ; on one occasion he could only prevail upon an instrument-maker to carry out a somewhat rough instrument by promising that no one should know who had made it.

Mr A. C. JOLLEY wrote that he considered the subject of Mr Rosenhain's paper one of the most important for discussion at the Convention ; he felt that all users of instruments would cordially agree with Mr Rosenhain's contentions, but he (Mr Rosenhain) had certainly treated the instrument-makers very gently. On the other hand, Mr Jolley does not consider it advisable that all instruments should follow heavy engineering practice ; instruments might be divided into two classes :—(1) those intended for commercial testing ; they are in constant use, and are required to perform certain prescribed tests rapidly and accurately ; and (2) instruments for research and delicate measurements of a special nature necessitating a skilled manipulator. For the first class Mr Jolley regarded the machine-tool as the ideal model, everything being devoted to accuracy and strength ; but in the second class, weight would be in some respects a disadvantage, particularly as such instruments are only required occasionally.

From a close acquaintance with instrument-makers Mr Jolley would confidently assert that most of them have simply followed conventional lines without considering mechanical principles ; as a consequence, time is constantly being lost in discovering and remedying defects in detail. The absence of steadying pins in fitted surfaces, micrometer screws in short nuts, or without efficient means of taking up back-lash, and structures built without considering thermal expansion are details of the daily experience of the experimentalist. The use of hollow steel cylinders for micrometer screws is well worth consideration by instrument-makers, since it allows of larger diameter without excessive weight, and also allows of more accurate fitting.

The question of embellishment and lacquer is also, Mr Jolley considers, an important one, for not only is much of the first cost of the instrument uselessly expended in this direction, but the bright finish so obtained sometimes occasions considerable trouble in the use of the instrument, and when such surfaces become worn and damaged in the legitimate use of the instrument, they actually detract from the appearance of the apparatus. In one case in Mr Jolley's experience, in working with a very expensive optical bench considerable time was lost at each setting in arranging screens to arrest the light reflected from the highly lacquered parts of the stands and pedestals, while in the same bench, owing to the absence of geometrical contacts, the axis could not be relied on, and had to be readjusted at each setting.

DIFFRACTION IN OPTICAL INSTRUMENTS.

By J. W. GORDON.

IN the geometrical representation of a beam of light there are two constituent elements—the rays and the wave-fronts.

The rays traverse the beam from end to end, and extend in one dimension only.

The wave-fronts lie athwart the beam, and are extended in the two remaining dimensions. The wave-fronts may be more exactly defined, for they are monophasal surfaces. A wave-front may accordingly be said to pass through all those points in the rays composing any beam which lie at a given optical distance from its point of origin.

It thus appears that the rays intersect the wave-fronts in a beam of light. From the nature of this intersection, the type of the beam may be determined. Thus the ray, where it intersects the wave-front, may be a normal to the surface of the wave-front, or it may meet it at an oblique angle. If the ray is a normal, the pencil of which it forms a part is normal, and we have the normal beam of ordinary light, which forms the subject of investigation in what is commonly called Geometrical Optics.

But when the angle of intersection is oblique, the beam is a beam of diffracted light.

The following diagram will make these definitions plain to the eye. In Fig. 1 ϵ is the source of light, LL is a lens by which the light is focussed and the beam defined, η_0 is the focal point of the
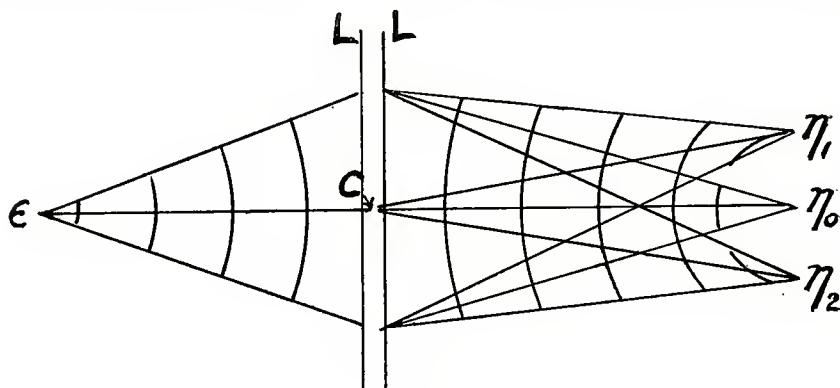
Fig. 1.

normal beam, η_1 and η_2 are focal points of diffracted beams, C is the optical centre. Three rays are delineated in addition to the boundary rays: ϵ C, the axial ray of the beam incident upon the lens, its prolongation $C\eta_0$, the axial ray of the transmitted beam, and $C\eta_1$, $C\eta_2$, the axial rays of the diffracted beams. It will be observed that the rays of the two normal beams (the divergent and the convergent) are normal to their wave-fronts, whereas the rays of the two diffracted beams pass obliquely through their wave-fronts. If any other rays be drawn upon the diagram, it will be found that with regard to them also the same rule holds. Thus the angular position of the rays with reference to the wave-fronts affords a criterion by which we may distinguish normal from diffracted light.

In Fig. 1 the diffraction phenomena (Fraunhofer bands) are exhibited outside the geometrical boundary of the normal beam of light. The phenomena which are named after Fresnel make their appearance within this boundary. Consider, for example, the case illustrated by Fig. 2. Here the wave-front which fills the aperture A—A, and is by it defined, is a spherical wave-front struck about the centre η. If now we trace the progress of this wave-front we can infer at once from general considerations that all parts of it will move forward with the same velocity and with symmetrical change of form, subject

only to an exception to be presently particularised. The general rule follows at once from the consideration that all parts of the wave-front are *ex hypothesi* subject to the same impulse. If then we take two points situated as are p and p_1, well in the path of the advancing wave, equidistant from and
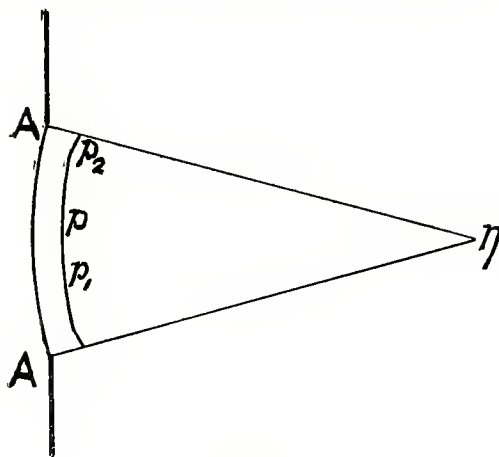
Fig. 2.
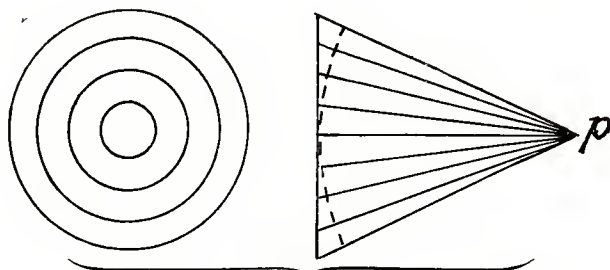
Fig. 3.

slightly in advance of it, we may safely infer that both will simultaneously receive the same impulse from it, for even if we assume that the impulse which each receives is derived from a small finite area, not from a mere point on the surface of the wave, yet even two such tributary areas drawn upon the wave-front would be optically indistinguishable from one another, and would stand each to its own object point in the same optical relationship. This may be illustrated by means of Fig. 3, in which we have a diagram illustrating the supposed wave transmission by plan and section. The plan is divided into concentric zones, and it is at once apparent from the section that the zones will roughly indicate the graduation in respect of phase on arrival at the point p of the light received from different parts of the area depicted. That is to say, all parts of any one zone will, within narrow limits of variation, send light of a particular phase, and the phase will vary in such wise as to correspond to the order of the zones, that received from the outermost zone being latest in phase, and that from the central region being earliest. Thus the larger the proportion to the whole light of light contributed from the outer zones the more retarded the phase of the resulting light. But whatever the result in the case of the point p the same result must of necessity be reached at the point p_1 (Fig. 2), for both are lighted by equal and optically indistinguishable areas upon the wave-front.

Recurring now to Fig. 2, and taking the case of a point situated as is p_2, just within the boundary of the beam, we see that different considerations arise. The tributary area on the illuminating wave-front will be mutilated by the edge which cuts down the aperture as in Fig. 4, so that the zones which tend to retard the resulting phase will be less effective, and the phase will consequently be accelerated. Thus the edges of the wave-front will tend to curl forward as shown (Fig. 2), and on the surface of the great wave a little wavelet will be formed with a tendency to travel inward towards the centre of the wave-front.

The formation of this wavelet will of course disturb the structure of the system and the tracing out in detail of all the consequences of this disturbance would be tedious. Suffice it then to say that as a

Fig. 4.

matter of observation a series of such wavelets can be seen within the beam forming ring systems with alternate bright and dark bands encircling one another in a concentric series on the surface of the advancing wave-fronts. Fig. 5 shows such a series of Fresnel rings, magnified about twenty-fold,
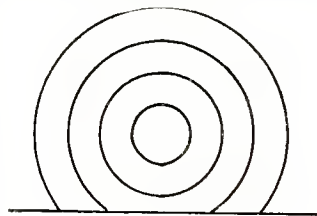
in the aperture of a focussed beam of light at a distance of 4 in. from the focal point, the semivertical angle of the beam (u) being $u = \sin^{-1} \frac{1}{220}$.

In order to render a complete account of the Fraunhofer phenomena, partially illustrated by Fig. 1, it is necessary to examine the region adjacent to the edge of the beam, but lying beyond that edge in planes other than the focal plane. It will then be found that, although the text books do not, as a rule, give any account of Fraunhofer rings, except in the focal plane itself, they are actually developed all along the course of the beam. It, will, however, be convenient to start from the basis of the commonly received doctrine, and therefore Fig. 6 may be taken as the starting-point. This diagram exhibits the
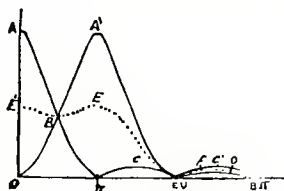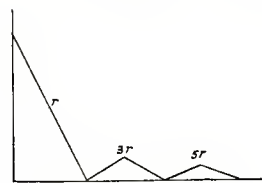
Fig. 5.   Fig. 6.   Fig. 7.

intensity curve of the Fraunhofer rings in the focussed image of a point of light, as given in Lord Rayleigh's article on the Wave theory, in vol. xxiv. of the *Encyclopædia Britannica*. Here it will be seen that the central region is enormously brighter than the region occupied by the concentric zones surrounding it, and it would seem natural to infer that it must be to such an extent the preponderating factor in the phenomena of diffraction that the rings surrounding it may, in practice, be left out of account. That, however, would not be quite correct, and a very obvious consideration shows that it would be, at least, a precipitate conclusion. For although the central region shows so much greater intensity of light than the outlying zones, it occupies, of course, much less space than they; and, in fact, a very little examination informs us that the total amount of light contained in any one of the rings bears a considerable proportion to the total amount of light contained in the central region, notwithstanding the great difference of light potential between them. A comparison of Fig. 7 and Fig. 6 will make this evident to the eye. In this last figure the heights at the successive maxima marked r, $3r$, and $5r$ are proportioned to 1, 3^{-1}, and 5^{-1} respectively, so that if a ring system were moulded having the sectional form shown by the curve, the separate rings would have approximately equal volumes, and the volume of the central cone would be very nearly equal to that of any one of the rings. A slight modification of the curve would make this equation rigorously good. But that slight modification would greatly complicate the geometrical structure of the curve, and therefore it is not considered worth while to introduce it into a diagram which is intended only to appeal to the eye.

Going back now to Fig. 6, we perceive that it denotes a distribution of the light among the different zones making up the Fraunhofer system, in which the central region preponderates, but not by any means in the great excess suggested to the eye. In fact the quantity of light contained in the innermost ring is a little more than three times the quantity contained in the next ring. Accordingly, we should expect to find that, if the system were shifted up the beam to a plane nearer to the aperture than the focal plane, the preponderance of the central region—the innermost ring, as it would then become— would be reduced, and that, as the experimental plane travelled away from the focal plane, this innermost ring would tend to exhibit diminished relative brightness as compared with the outer rings. Such, in fact, would be the theoretical result of substituting another plane for the focal plane, and calculating the intensity of the Fraunhofer rings in this new plane by the accepted formulas.

Observation, however, does not confirm this theoretical conclusion. If we focus the eye-piece of a telescope upon a plane higher up the beam than the focal plane, we shall observe in addition to the Fresnel rings shown in Fig. 5 at least one Fraunhofer ring. This will be the innermost ring, and in favourable circumstances one or more of the Fraunhofer rings surrounding it may also be seen. But it

is noticeable that however high upon the beam we select our plane of observation, we do not find that this innermost Fraunhofer ring tends in fact to sink to the calculated level of brightness. Its pre-eminence is just as marked at a distance from the focus as it is at the focal plane itself. An enlarged photograph actually made from such a beam in the same position from which Fig. 5 was drawn is given at Fig. 8. In this case, only the innermost Fraunhofer ring is strong enough to be visible at all, but it is visibly quite equal in brilliancy to the Fresnel rings. It is distinguishable from the Fresnel rings by several marked features—that is to say, it is broader and of a somewhat different colour.
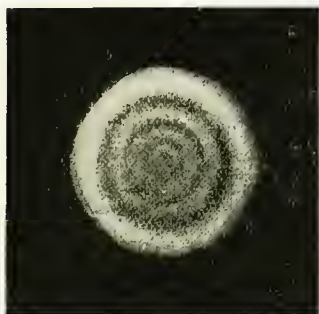
Fig. 8.

These marked departures in the behaviour of the Fraunhofer system from the theoretical standard lead necessarily to the suggestion that some more precise theory of them is to be desired. An attempt has recently been made to supply this desideratum, and I had myself the honour, in December last, of bringing it forward in a paper submitted to the Royal Microscopical Society. The informing idea of this mode of attacking the problem is to treat the external surface of the beam, and not the wave-front, as the radiant surface which is the source of the diffracted light. The mathematical problem is discussed in that paper,[1] here it will suffice to say that a formula is found which yields results entirely congruous with the observations to which attention has above been drawn, and which suggests a consideration of even greater importance. That is to say, there is reason to think that the system of Fraunhofer rings is distinguished not only by alterations of brightness, but also by a gradual change of phase. In fact, such a graduated succession of phases is necessary to explain the power which the Fraunhofer ring has of moving across the horizontal plane as it contracts upon its centre, and this consideration serves also to show what the nature of the graduation must be. Suffice it here to say, without detailed discussion, that the result may he expressed at least approximately, by supposing that this ring of diffracted light has the form of a conical wave-front closely adherent to the surface of the beam and stretching out from it in a direction normal to its surface. Fig. 9 is a diagram of such a beam showing in axial section the normal beam itself with its internal Fresnel rings and one external ring, the innermost of the Fraunhofer series. It will be observed that the Fresnel rings all disappear before the focal plane is reached, but the Fraunhofer ring persists, and in the focal plane itself forms the so-called false disc of the antipoint. It now appears why the Fraunhofer system is so much more practically important than the Fresnel system of diffraction bands in the theory of optical instruments. The Fresnel rings disappear one by one in the course of the beam, and the last is thus extinguished at a point short of the focal point itself, but the Fraunhofer ring persists and travels forward into the focal plane, where it forms the circle known as the false disc. Hence the power of resolution of any optical instrument depends upon the behaviour of this Fraunhofer ring, whereas the Fresnel rings, which merely redistribute the light within the beam, have no effect upon its focussing properties.
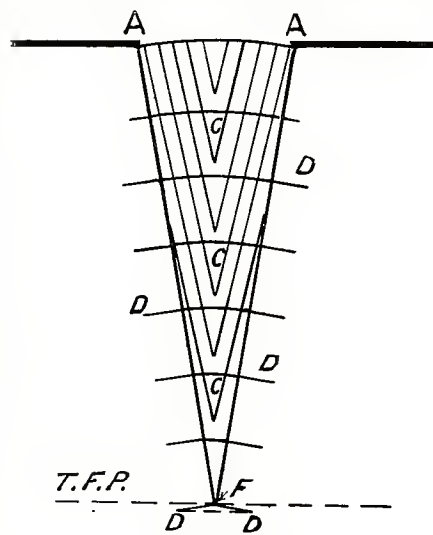
Fig. 9.

A A is the aperture defining the beam. F the focal point. The circular arcs struck about the point F represent the wave-fronts of the normal light. C C C are traces of the dark intervals between the Fresnel rings. D are the conical wave-fronts of the Fraunhofer ring. TFP is the theoretical focal plane.

[1] See *Jour. Roy. Mic. Soc.* (1905), p. 1. By an unfortunate printer's error on p. 29 in three places, and on p. 30 in one place, the expression $\frac{\sin n\theta}{}$ is written $\frac{\sin n\theta}{}$. The mathematical reader will easily correct the error.

A glance at Fig. 9 suffices to suggest a fact of capital importance in the theory of optical instruments, and of some practical significance in connection with their construction. For it is evident that the Fraunhofer ring which has contracted into the conical wave-front constituting the false disc in this diagram will not cease at that point to contract upon itself. It is the property of a wave-front to travel onward in a path normal to its own surface, and therefore we conclude that this conical wave-front will so travel forward, condensing still more upon itself in the act of so doing, and, after turning inside out, expanding again on the other side of the point of greatest condensation.

In order to appreciate the phenomena which will result from these evolutions it will be convenient to consider them with the aid of a diagram. Fig. 10 will serve this purpose. Here, as in Fig. 9,
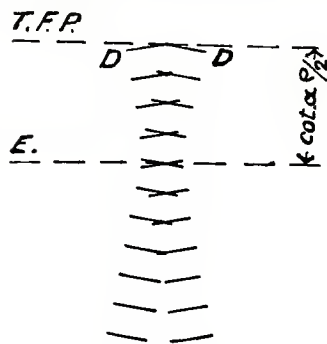
Fig. 10.

T.F.P. is the theoretical focal plane, and D is the conical wave-front formed at the focal point by the innermost Fraunhofer ring. The further path of this conical wave-front is traced through the plane marked E, in which it is most condensed and forms a disc, having only half the diameter which the accepted theory assigns to the false disc. Now it will be fairly obvious that if this diagram correctly represents the structure of the beam, the effective focus must be in this plane E, and not in the theoretical focal plane, for here the false disc has half the diameter and double the brightness that it has in the theoretical plane. Here, then, we have a sufficiently pronounced result by which to test this hypothesis by means of observation, for the position of the effective focal plane can be determined with considerable nicety.

Before continuing the discussion on these lines it will be useful to consider what will be the most convenient form of test to apply. And here a very simple test may be suggested. It is quite obvious that the distance between the two planes, T.F.P. and E, must depend upon the angle which the conical wave-front D makes with the focal plane. Let us call this angle α. Then a very simple geometrical construction shows that the distance between these two planes measured upon the optical axis, which we may write Δ is

$$\Delta = \cot \alpha \frac{\rho}{2}$$

where ρ is the radius of the false disc measured in the theoretical focal plane. We may take it that ρ will have a value very nearly equal to

$$\rho = \frac{\lambda}{2 \sin u}$$

if u be written for the divergence angle of the normal beam.

We thus obtain for Δ the value—

$$\Delta = \frac{\cot \alpha \ \lambda}{4 \sin u} . \qquad \qquad . \qquad . \qquad (1)$$

And if we assume that the conical wave-front actually has the form given to it in Fig. 9, it will follow that the angle α = the angle u.

On that assumption—

$$\Delta = \frac{\cos u}{\sin ^2 u} \frac{\lambda}{4} . \qquad \qquad . \qquad . \qquad . \qquad (2)$$

Now, it is quite possible that $\alpha = u$ is not rigorously true ; but, on the other hand, it is certain that if the hypothesis now under discussion be correct, these two angles must be approximately equal to one another. Equation (2) will therefore furnish materials, at least for rough tests if not for exact calculation. Let us consider what is its significance.

In the first place, it is clear that in the case of a beam of wide angle Δ will be very small. Putting the limiting case $u = \frac{\pi}{2}$, we have $\Delta = o$, and in that case, therefore, the effective focus will actually

coincide with the theoretical focus. Even if we take a beam with a divergence angle of $30°$, the distance $\Delta < \lambda$—an insensibly small quantity. Not until the beam is reduced to a divergence angle of $u = \frac{4}{5}$ of $1°$ will the displacement of its focal plane for green light amount to $\frac{1}{100}$-in. Thus, the phenomenon is not one to force itself upon the observer's attention when instruments of moderate magnifying power are in use.

But when images are formed upon a very high scale of magnification, recourse is, of necessity, had to beams of extremely small angular magnitude. Even with the use of immersion objectives, the divergence angle of the image-forming beam cannot well be greater, or, at best, much greater, than $u = \sin^{-1} \frac{n}{M}$, where $M =$ the magnifying power of the combination, and $n =$ the refractive index of the mounting medium. If, then, we take $\frac{n}{M}$ to be the sine of this divergence angle, we may write equation (2) in the following form :—

$$\Delta = \frac{\cos u}{n^2} \frac{M^2 \lambda}{4}.$$

And as we are now concerned with cases in which M is large and u, therefore, very small, we may, without sensible error, write this last expression thus :

$$\Delta = \frac{M^2 \lambda}{4n^2}. \qquad . \qquad . \qquad . \qquad (3)$$

It now becomes evident that the microscope or telescope arranged for giving photographs upon a high scale of magnification affords the means of bringing this hypothesis to a test. We have, for example, only to arrange either instrument for very attainable magnifying powers, and we shall get quite measurable displacements of the focal plane. For example, assume a magnifying power $M = 100$, and light having the wave-length $\lambda = \frac{1}{50,000}$-in. Then we have, in the case of an object mounted dry—

$$(4) \qquad \Delta = \frac{M^2 \lambda}{4} = \frac{1}{20}\text{-in.}$$

This, of course, is an easily measurable magnitude, and thus it appears that the hypothesis may, without difficulty, be subjected to a crucial test. I say that the hypothesis may be subjected to such a test. For, if it turns out that the focal plane is shifted according to any such law as has now been deduced, I take it that no optical theory at present before the world will serve to explain the phenomenon. What, then, is the result of experiment?

First, let me refer in general terms to certain well-known phenomena which may be put in evidence, if not as conclusive, at least as being singularly consistent with the view now put forward. Conclusive they cannot be, for the simple reason that they have never yet been exactly measured, and, of course, only exact measurements can afford any really critical test of a mathematical law.

But under allowance for the uncritical nature of these preliminary observations I may refer to a fact with which every photographer is familiar, namely, that for really critical focussing it is necessary to focus with the particular stop with which you intend to operate during exposure. It is very convenient often to use a large stop for focussing in order to secure strong illumination of the focussing screen when it is intended to use a small stop for exposure, and in rough work this plan succeeds, but if very sharp focussing on a particular plane in the object is desired, the same stop must be used in both operations. This rule is commonly explained upon the ground that different zones of the objective are corrected for slightly different depths of focus, and there may be some truth in that explanation. But, on the other hand, it now appears that, however perfect the correction, a change of stop may alone be the cause of a disturbance of the focal plane quite sufficient to impair a critically fine picture, and here at least is food for thought.

Then there is another observation worthy of note in this connection, and this time it is one which must have forced itself upon the notice of every microscopist. When working with high powers he has observed that the focus is immediately disturbed by any change in the adjustment of the substage

condenser, and that if in this way the angle of the light is altered even slightly, a corresponding readjustment of the focus becomes necessary. This on any known theory of the microscope is very hard to explain. It cannot, like the disturbed focus of the photographic camera, be set down to faulty correction, for the disturbance ensues upon changes too slight for that in the angle of the condenser beam, and, so far as I am aware, no other explanation is upon any accepted theory forthcoming. But if the image plane is displaced so much as equation (2) indicates by a reduction in the angle of the image forming beam, then it is easy to see that anything which produces such a change, even though to a very slight extent, will necessitate a corresponding readjustment of focus.

These are matters quite commonly known. To them I may add one other of the same class which has come particularly under my own observation. Some two years ago I was led by the reading of Helmholtz's paper upon the theoretical limits of the resolving power of the microscope to make the experiment of interposing a diaphanous screen in the image plane of the microscope for the purpose of increasing the diameter of the Ramsden circle. The object in view was to get rid of those intrusive shadows which, when high magnifying power is used, are cast by specks of dust upon the ocular, or by opaque bodies in the eye, and which, projected by the instrument upon the object, are seen as blemishes in the image. This object was fully accomplished by means of a glass screen ground to a fine greyness and kept in oscillatory motion sufficiently rapid to render its grain invisible. But incidentally another result was observed for which at that time I could find no explanation, and which the Helmholtz theory did not at all lead me to expect. That is to say, the image seen upon the oscillating screen showed much greater crispness of definition than when seen as an aërial image, and also—but this did not appear at all unintelligible—I thought that it put much less strain upon the eye. Now why should the interposition of a screen improve the definition of the picture? That was a result that could not at all have been foreseen. Indeed practical considerations pointed to the opposite conclusion, and theory showed no countervailing reason. The screen could not reduce the dimensions of the antipoint, and it did introduce a disturbing element in the shape of a new adjustment to be made, for, of course, it would destroy the picture altogether if it were not itself mounted exactly in the image plane of the instrument.

It is, however, now quite evident that if the focal plane is so greatly displaced as Equation 3 indicates by the narrowing of the Ramsden Circle which goes with high magnifying power, some marked improvement is to be expected from the interposition of the screen. Thus it seems quite reasonable to assume that a displacement of the focal plane within the eye of but little more than $\frac{1}{20}$ in. would carry it beyond the range of accommodation, and that under that condition it might be impossible to throw upon the retina a truly focussed image at all. Now Equation 4 shows that this displacement of $\frac{1}{20}$ in. would occur with a magnifying power much less than what was actually employed in the experiments now under notice, for it is there shown to attend upon a magnifying power of 100 diameters. This, of course, signifies a real magnifying power of that figure, as magnifying power, for example, is reckoned in photo-micrography. If an object mounted in air is represented by an image on the retina of its own size, it is conventionally taken to be magnified ten times, and if the object so depicted on the retina is mounted in glass the magnification is reckoned as fifteen times. For the purpose of the present computation, therefore, we must make allowance for this conventional factor, which may be anything from 10 to 15, according to the mounting medium in use, and, stated in the conventional way, the result now under discussion is that an image gets beyond the focussing power of the eye if it is magnified by a dry lens much over 1000 times, or by an oil immersion lens much over 1500 times. Now the experiments of which I speak were made with magnifying powers varying—according to the conventional reckoning—from 5000 to 10,000 times, and on the present hypothesis that must certainly have thrust the focal plane back well behind the retina, even when the extreme effort at accommodation had been made by the eye. The introduction of the screen, on the contrary, by increasing to normal dimensions the pencil of light admitted to the eye, would at once bring back the focal plane to its normal position and enable the retina to receive an accurately focussed picture. Thus both the improved definition and the greater ease with which the picture was seen are quite simply explained upon the present hypothesis.

So much may, perhaps, suffice in the way of a general statement concerning the class of phenomena with which in this connection we have to deal. More decisive tests may be looked for from the application of the theory to micro-photographic apparatus, and such tests are already in progress. Indeed I thought a few days ago that I had secured decisive results in that way. But in fact the experiment is not so easy as I imagined, and at the moment of writing I am not able to bring forward any satisfactory evidence of this description either for or against the present hypothesis. No doubt the experiment will be made and a decisive result attained when the mechanical difficulties are overcome.

It will, I fear, be thought that I owe this Society some apology for bringing forward so much immature matter in this present paper. And indeed I am very sensible that I must do something to satisfy that feeling. May I then add in a very few words the explanation that it is not by my choice, and indeed I hope and think that it is not by my fault, that the discussion of this subject has taken the present form. It would have been much more agreeable to me to found myself upon the authority of the great writers who have already treated of diffraction than to ask you to fall back upon first principles and to work with me through a discussion of elementary topics. But the choice was not open to me, for the matters which we have actually discussed are, so far as I can judge, by much the most practically important matters connected with the bearing of diffraction upon the behaviour of optical instruments, so that they cannot be passed by in this connection, and yet, by a curious mischance, they have been, so far as I am aware, entirely overlooked hitherto. The fact seems to be that the theory of Fraunhofer diffraction has not hitherto been studied to very good purpose. In recent times the study of the theory of diffraction in its practical aspects has been bound up almost exclusively with the study of the diffraction grating, and as the spectroscope does not involve the use of very high magnifying powers, the phenomena associated with high magnification have come but little under the notice of those writers who have dealt with diffraction in connection with the theory of the spectroscope. It thus happens that the Fraunhofer phenomena have been almost exclusively discussed as a branch of the higher mathematics, and although very beautiful mathematical theories have been elaborated in the course of its discussion, they have had no very direct relation to any matters of practical importance. This seems to be the reason why the theory of the Fraunhofer bands is in a very undeveloped condition, and this is my apology—as I hope, my sufficient apology--for occupying your attention on this occasion with matters which, although of great practical importance, are very imperfectly understood.

Mr J. Rheinberg understood Mr Gordon to say that the diffraction rings change in phase gradually and continuously, not suddenly. He believed that a change of phase in diffraction rings was first discovered by Airey, and that it was to be found in the calculations made by Schwerd. The phase of some of these rings was not very difficult to determine experimentally. Recently Mr Conrady read a paper at the Royal Microscopical Society, in which he showed that there was a change of phase in the rings; but the change of phase was not a continuous one, but changed suddenly. He showed that any change of phase must be an exact reversal, and certain experiments which Mr Conrady and Mr Rheinberg were able to make with the diffraction microscope confirmed that view. It was true that these experiments were made entirely with gratings—fairly coarse gratings. He tried to repeat them with single slits; but not having sufficient light available at the time, was not able to do so. "How could," he asked, "spherical surfaces give rise to a conical wave-front?" Mr Gordon calculated the phase at the particular point he wished, and obtained this result. But still it seemed difficult to see how anything approaching the shape of a conical wave-front could come about. Supposing he had obtained a conical wave-front in this way, was it fair to assume that that wave-front would maintain its shape when it was propagated further, because the conditions were entirely different from the case of a spherical wave surface, where the phase changed regularly. It might be useful to point out that if gold leaf, with a very fine perforation on it, were taken and placed as an object on the stage, using a one-inch objective, when the microscope was racked up and down, the internal and external rings were obtained in a beautiful manner.

Mr S. D. CHALMERS had hoped to submit Mr Gordon's conclusions to a definite experimental test, but the difficulties that were indicated in the Paper on "Aberrations" have so far made it impossible to obtain definite results. He hoped, however, by a series of careful measurements of aberrations, to be able to allow for their effect, and verify or disprove the theory.

Mr GORDON, in reply, said.—With regard to the points which Mr Rheinberg has put, I do not think there is any doubt that the change of phase is a gradual change. A very general consideration seems to show that that must be so. Mr Rheinberg suggested that a conical wave-front would not be propagated as a conical wave-front. I daresay it would not; and no doubt, as he says, it is very difficult to determine theoretically what would be the exact result of the propagation; but if we have an effective conical wave-front, it will condense upon itself, and we shall have a long white dot occupying the whole of the axis of the cone. That can quite easily be observed; it does happen, and is observed with great facility. Then, where the successive rings are separated by dark bands, we shall also have the sudden introduction of black dots, and that also can be observed. Although I do not suggest I am in a position to answer the question in general terms as to how a conical wave-front is propagated, it is propagated as a cone within narrow limits, but limits sufficiently extended to be of very great use. The integration is carried over the conical surface only; it is conducted over the whole of the cone, but one does not have to calculate it over the whole of the cone, because the ring is symmetrically placed with regard to the cone, and therefore, if you integrate over that part of the cone which subtends any part of the ring, you get one part of the ring, and you assume that all the parts are alike.

A SIMPLE METHOD OF PRODUCING ACHROMATIC INTERFERENCE BANDS.

By JULIUS RHEINBERG, F.R.M.S.

THE following is an account of some experiments on problems connected with microscopic vision which have led to an unexpected result in another direction. They were made with one of the so-called Demonstration Microscopes, devised by Abbe many years ago, which was kindly lent to me by Messrs Carl Zeiss of Jena. The instrument is on the table, and, as may be seen, it embodies in a practical form the view taken by Abbe, that a compound microscope may be considered to consist of a simple magnifying lens, to convert the divergent rays from the object into parallel pencils, and a telescope to bring these parallel pencils to a focus, and so to produce the image of the object.

In the Demonstration Microscope, the part corresponding to the simple magnifying lens, which for the future will be referred to as the objective proper, is composed of two long focus lenses mounted about 8 inches apart in a tube, and having an equivalent focal length of about 10 inches. By this means the parallel pencils to which any point in the object-plane gives rise are produced in reality. These parallel pencils are then brought to a focus by the actual telescope which forms the extension of the instrument. Between the objective proper and the telescope there is a considerable space, and it is within this space that the upper focal plane of the objective falls. As is well known, it is in this plane that the diffraction spectra of gratings, or other objects, are formed, provided the object is illuminated by plane-parallel light, and as the Demonstration Microscope was primarily designed for the purpose of studying the relation between these diffraction effects and the image in the view-plane (which in this instrument is the image-plane of the telescope), provision is made for viewing these diffraction spectra, when desired, by means of a simple lens. This lens, as well as the telescope, is arranged on a pivot, so that both can be swung in and out of action alternately.

It remains but to state that the illumination of the object by plane parallel light is secured by projecting the image of a flame on a micrometer slit placed in the back focal plane of the condenser, the latter being used, in fact, exactly like the collimator of a spectroscope.

The ordinary phenomena of resolving power in accordance with the Abbe theory can, of course, be admirably shown by this instrument. We place, for instance, a grating on the object stage. By means of a hinged lens used as a simple magnifier we view the spectra formed by the grating in the upper focal plane of the objective proper; we block out all the spectra in this plane except the central one, and we find that under these circumstances the lines of the object grating, when seen through the telescope, are not resolved. If, however, we admit two spectra, we do get resolution of the object, but according to whether the spectra are consecutive ones, or whether intermediate spectra have been blocked out, so does the image show the correct number of lines, or a duplication, triplication, etc. The chief point about these well-known experiments is, of course, that the image results from the reunion of rays
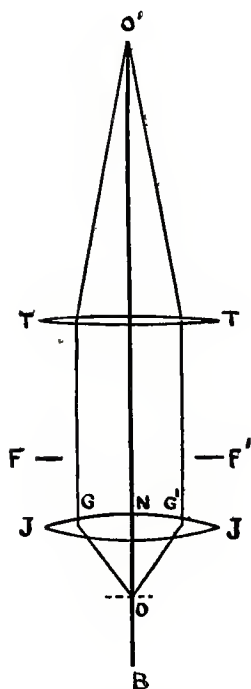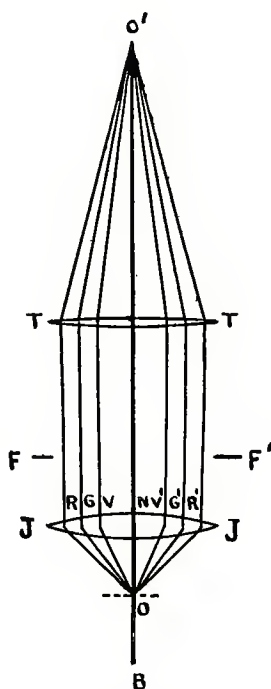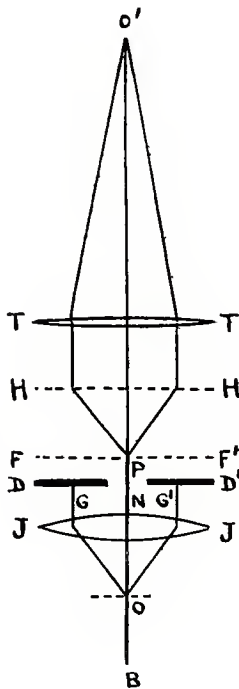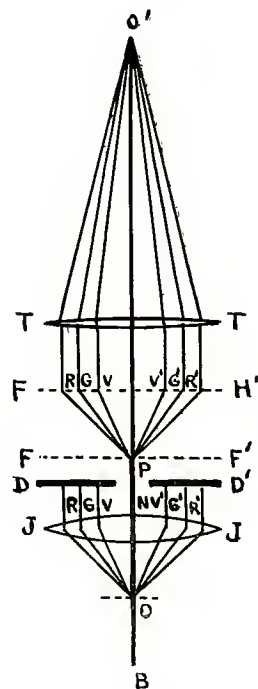
diffracted by the object, and that, in the case of a regular grating, at least two of the maxima must pass the objective if any defined appearance of structure is to be seen in the image.

And now we come more particularly to the subject of this paper. The question suggested itself to me :—

What would happen if the spectra in the focal plane of the objective, caused by *diffraction of the object*, were replaced in some way by *precisely similar spectra* produced in some other way, and how could this be attained?

The diagrams show the question reduced to its simplest form. At O (Fig. 1) we have the object grating which by diffraction splits up the incident ray BO into three parts, viz. the normally diffracted or dioptric ray ON, and the diffraction rays of the first order, OG and OG¹. These, after

passing through the objective proper in the plane JJ, issue parallel to one another, passing the upper focal plane FF, till they arrive at the telescope objective in the plane TT, which reunites them in a focus at O^1 in the image-plane.

Fig. 1 assumes monochromatic light; Fig. 2 represents the same with white light, so that the rays diffracted by the object proceed at different angles from O, according to the colour: OV, OG, and OR represent the diffracted violet, green, and red rays respectively. The point to notice specially is that *all rays* between the objective planes JJ and the planes TT are parallel, and that this applies to rays of all colours. Indeed, it is because the rays of all colours proceed parallel to one another, whilst at the same time they are spaced at a distance from the central ray, strictly in accordance with their wave-length, that the picture formed by the telescope in the image-plane is achromatic.

The problem to be solved was, after cutting off the spectra VGR and $V^1G^1R^1$ formed by the object, to produce precisely similar spectra in some other way *after* the ray BON has passed the object. What we want to do is indicated in Figs. 3 and 4 assumed to be for monochromatic and white light respectively. In these figures DD^1 shows a diaphragm cutting off the rays diffracted by the object, whilst laterally diffracted rays PV and PV^1, PG and PG^1, and PR and PR^1, representing the violet, green, and red rays respectively, are shown proceeding from the point P, situate in the upper focal plane of the objective. These rays are shown as being subsequently rendered parallel to one another, so that they may re-unite in the point O^1 in the image-plane, just the same as the rays in Figs. 1 and 2, and herein of course lay the difficulty of the problem. I ultimately hit upon a device by which this could be effected, viz. *that of passing the parallel beam through two diffraction gratings of equal pitch.* These are indicated in Figs. 3 and 4 by the dotted lines FF and HH.

Consider for a moment the action of two such gratings placed the one behind the other.

Let the ray AB (Fig. 5) impinge on the grating G^1, and let BC be the dioptric or normally diffracted ray, and BD one of the diffracted rays of the first order to which the grating gives rise. When the ray BC meets the second grating, it is again split up into several proportions, one of them (CE) proceeding in the original direction. When BD meets the second grating G^2, this also is split up, the dioptric portion continuing in the direction DH, and the first diffracted rays proceeding in the direction DK and DK^1. But as the grating is of the same pitch as the other one, the angle between DH and DK must be the same as the angle between BC and BD, so that DK is parallel to AB or CE,—that is to say, part of the incident light which was diffracted off at a particular angle by the first grating has been again rendered parallel to the incident ray, and consequently also parallel to the transmitted dioptric ray which has not had its direction changed.

And as this reasoning applies equally for rays of all colours and for diffracted rays of any order, it is clear that they *all* issue parallel to one another, the only difference being in their distance from the central or dioptric ray. This distance from the central ray for diffraction rays of different colours, as will be seen, is strictly proportional to their wave-length, and the peculiarly interesting feature now presents itself that this proportionality is independent of the distance separating the two gratings, for, as may be seen in Figs. 6 and 7 (in which the violet rays (V) are represented by ordinary and the red rays (R) by dotted lines), the ratio of CV to CR does not depend on the position of the second grating, but solely on the angles CBV and CBR. So we have the means at hand —

1. To obtain parallel rays of light of different colours, spaced in accordance with their wave-length, precisely as occurs in the back focal plane of the objective proper, in the Demonstration Microscope, where the diffraction is caused by the object-grating itself.

2. We have the power, without disturbing the parallelism of any of the rays, to vary the width between the central ray and the diffracted ray of any order, so that we can make this width the same as if the rays had been diffracted by the object.

That the dioptric and diffracted rays of any one colour will be capable of interference in the one case just as in the other is obvious, for in both cases they have been derived from the same source.

But we now come to two points of difference :—

1. In the case of the microscope the relative intensity of the dioptric and the diffracted beams in the back focal plane of the microscope depends upon the pitch of the *object*-grating.

In the double-grating method we are considering not only do the relative intensities of the dioptric and diffracted beams depend upon the pitch of the particular gratings used, but they vary in quite a different ratio. For, suppose for a moment that the violet ray impinging on the first grating has an intensity 100, and that the relative intensity of the dioptric and diffracted ray of the first order is as 50 to 20 when it has passed the first grating, then, when the rays pass the second grating, the components of the same parallel to the incident ray have a relative intensity of 25 to 4. This is seen on reference to Fig. 7, in which the intensities of the rays are written alongside. It is evident that the parallel components emerging from the second lens are diminished in intensity, according to the square of the rate at which they are diminished on emergence from the first grating.

2. In the microscope the diffracted and the dioptric rays from an object point on the axis of the instrument arrive in the back focal plane in the same phase, because the optical path-length is the same.
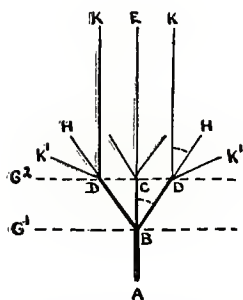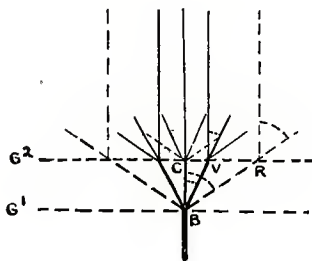
Fig. 5.   Fig. 6.   Fig. 7.
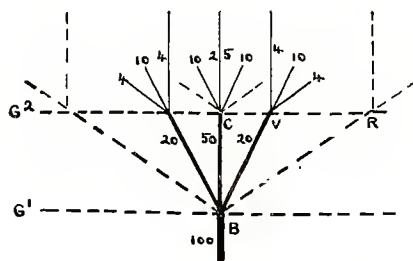
In the double-grating method the phase differs according to the difference in the path-length between BC and BV (Fig. 6) which varies according to the distance between the gratings.

These two points of difference we can get rid of by making use in both instances of the two diffraction bands of the first order, *i.e.* the one on the left and the one on the right of the central beam, and blocking out everything else. For in both cases these two bands will have an equal light intensity and an equal optical path-length.

The method in practice, therefore, is as follows:—Two Thorp gratings (Fig. 8) on plane parallel glass, of 15,500 lines per inch, are placed in suitable frames attached to a bar in such a way that one of the frames can, by means of a screw adjustment, be made to travel backwards and forwards, keeping strictly parallel to the other. The gratings should be placed with their line surfaces towards one another, as by so doing all the rays in question pass through the glass normally, and we avoid the complication of dispersion of the glass. The gratings themselves rest on two micrometer screws (SS, Fig. 8), by which means their tilt can be altered so that one ruling may be adjusted to absolute parallelism with the other. This is important, as, if the lines on the two gratings are at the slightest angle to one another, the effects vanish, and numerous trials I made failed for want of the precaution to have a delicate adjustment for this purpose. One frame should be provided with clips or with other suitable means for

retaining small strips of metal, or celluloid, of various widths immediately in front of the glass, such strips (M, Fig. 8) being used to cover up the central beam of light, or such other part of the light as for the purposes of experiment should be obliterated.

The stand, with frames and gratings, is placed approximately in the back focal plane of the objective proper of the demonstration microscope, so that all the *parallel* rays passing through it are focussed by the telescope in the view plane. Of the rays which are diffracted in other directions by the second grating we need take no account, as they proceed in highly divergent pencils, and are, for the most part, absorbed by the black lining of the telescope. Any part of them which does get through the optical system is spread evenly over the whole of the field and merely produces a very slight haze.

Let us suppose that previous to placing the Thorp gratings in the path of the optical system we had been looking at the image of a grating on the object stage (with lines and spaces of equal breadth), and that in the back focal plane of the objective proper we had blocked out all except the first diffraction spectra on the right and left of the central beam. The image would, of course, show a line system with double the number of lines of the grating, the gradation of light and darkness being in accordance with the sine curve. Then our next step would have been to uncover the central beam in the back focal plane, and, upon placing the Thorp gratings in position, with their lines parallel to the object stage grating, and viewing the focal plane with the hinged lens, we should see the spectra formed by these gratings, as well as those produced by the object grating. Next we adjust the distance between the Thorp gratings till the spectra caused by these exactly overlap those caused by the object grating. Then, upon covering up the object-grating spectra before they reach the Thorp gratings, leaving only the central beam to pass through these, and likewise covering up the central beam after its passage through the Thorp gratings, so that only the first diffraction spectrum on each side produced by these latter is left to produce any effect, we bring the hinged telescope in position and can compare results. What we see is a precisely similar image to that seen before. The field is covered with black and white lines, which merge into one another, the number of lines corresponding to twice those of the object grating, as before. At first sight, therefore, it appears that we have actually found a means of evading Abbe's law that no image of an object can be produced if only one of the diffraction spectra[1] which the object grating gives rise to, reaches the view plane. This idea is, however, dispelled by a very simple trial, for we find that *we may rotate the object grating on the stage* without altering the direction or pitch of the lines in the view plane. When the object grating is rotated, its true image or absence of image is seen in every respect in accordance with what the Abbe theory leads us to expect, but superimposed on this is the interference image obtained by means of the Thorp gratings. Therefore when the lines of the object grating and the Thorp gratings are not parallel to one another, the image shows two sets of lines crossing each other at an angle.

It is but a short step from this to remove the object grating altogether. We then find that this makes no difference with respect to the lines produced in the view plane of the microscope by the agency of the Thorp gratings, and that we have a considerable portion of the field covered with black and white interference lines.

What have we learnt from these experiments? So far as the microscope image is concerned, they are merely an object-lesson as to the correctness of Abbe's laws as to resolving power and the nature of the image, inasmuch as they show what difficulties beset the path of any attempt to evade these laws, even in so artificial a manner as the one described.

The result, therefore, notwithstanding that in an isolated instance by conforming to certain stringent conditions we have apparently, though not really, succeeded in evading one of these laws, is a strictly negative one; and were this the whole outcome of the experiments, although perhaps they might not have been without interest to Microscopical Societies, I would not have considered them of sufficiently general interest to bring to the notice of the Optical Convention.

But you will see that these experiments, made for a specified purpose, have led to another result,

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[1] The general term *Diffraction Spectrum* includes the central beam or zero spectrum.

which I hope may be of more general interest, viz. the production of achromatic interference bands in a way at once simple and inexpensive, and which we may call the "Double-Grating" Method.

Can these be applied to any practical purpose? That is a matter which I must leave to those who have made a special study of measurements by interference methods; but it suggests itself to me that the convenience of being able to use white instead of homogeneous light for the production of interference bands, and the facility with which the spacing of the bands may be varied, may not be without its practical utility, apart from any theoretical interest which may attach to this method.

Mr STANSFIELD was exceedingly interested in Mr Rheinberg's paper, especially the part in which he referred to the two gratings, because he produced the same effect some time ago by using one grating and a reflecting mirror. He obtained a reflection of the grating in the mirror, so that he had practically two gratings, one behind the other, of exactly equal pitch, and exactly parallel to one another. You seem to obtain in that way just the conditions which Mr Rheinberg required for giving his bands. The bands, as he saw them in that case, were not quite achromatic. He could only see about twenty-five. The central ones were nearly free from colour, but they soon became coloured on going towards the edge. At the time he was showing this with some other effects to the Manchester Literary and Philosophical Society, he compared them to the very simplest cases of achromatic bands produced in a similar way with which we were familiar, namely, the band produced when you look through one row of railings on another parallel row of railings at a short distance from them. In that case you saw a system of black and white interference bands, perfectly free from colour and very distinct. When you reduced the size down to $\frac{1}{14,000}''$, the phenomena become more diffraction phenomena, and the effect was not quite the same. Mr Rheinberg had elucidated some of the difficulties that he had to deal with.
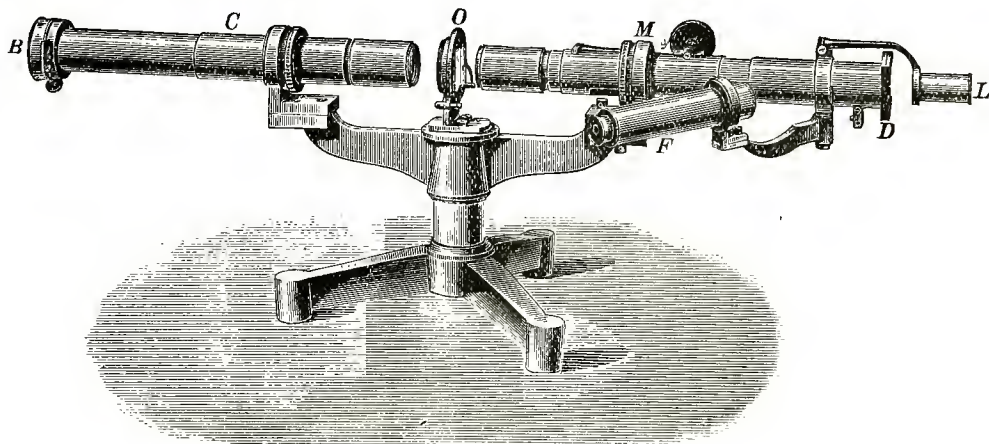
Abbe Demonstration Microscope.

THURSDAY, JUNE 1st

SECTION I.

THE EARL OF ROSSE in the Chair.

THE MICHELSON ECHELON DIFFRACTION GRATING, AND THE LUMMER PARALLEL PLATE SPECTROSCOPE.

By F. Twyman.

The theory of the Michelson Echelon Diffraction Grating has been several times clearly set forth, so that it will be better for me, in the short time at my disposal, to take it that all present are familiar with this part of the subject, and to touch on it as briefly as possible.

It consists (see Fig. 1) of plates of parallel glass of equal thickness, superposed in step form ; the beam of light passes through the mass emerging at the small exposed surfaces of the plates.
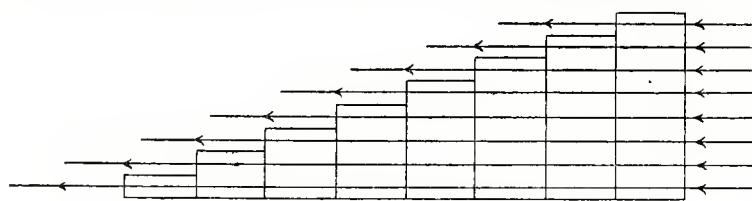
FIG.2. TRANSMISSION ECHELON.

Fig. 1.

The incident beam of parallel light is produced by a collimator, and the light is received in a telescope, as in an ordinary spectroscope. The beams emerging from the successive apertures will then be relatively retarded by equal amounts, and will produce spectra by interference.

The expressions for the dispersion, separation of successive orders of spectra, etc., will be found in the *Astrophysical Journal* for June 1898, in the *Journal de Physique*, Third Series, Tome viii., 1899 ; and also in a little pamphlet compiled by myself, and issued by Messrs Adam Hilger, Ltd., with copies of which they will be pleased to supply any one who cares to apply to them.

We are at present only concerned with the demands which the theory of the apparatus makes on the skill and patience of the optician.

It will be easily seen that if t be the thickness of the plates, and μ the refractive index for the radiation (supposed perfectly homogeneous) which is to be submitted to the action of the echelon, then the relative retardation of the beams from successive steps will be $(\mu - 1)t$. If $(n + 1)$ be the number of such successive beams under consideration, the relative retardation of the extreme beams will be $n(\mu - 1)t$.

Let Δt represent the greatest error from the mean thickness t, which is permitted to remain in the plates. Then, if the total number of plates be N, the greatest error in retardation will be caused by $\frac{N}{2}$ thin plates, all of the maximum error, followed by $\frac{N}{2}$ thick ones, also all of the maximum error, and will amount to $\frac{N}{2}(\mu - 1)\Delta t$.

According to Lord Rayleigh's well-known rule this must be less than $\dfrac{\lambda}{4}$ for the echelon to perform

perfectly, *i.e.* Δt must be $< \dfrac{\lambda}{4} \times \dfrac{2}{N(\mu - 1)}$ *i.e.* $< \dfrac{\lambda}{1 \cdot 16N}$ for a light flint of refractive index $= 1 \cdot 58$.

If the number of plates N be 10, the error in thickness should not be permitted to be greater than $\dfrac{\lambda}{11 \cdot 6}$ and if $N = 20$, not greater than $\dfrac{\lambda}{23 \cdot 2}$ and so on. This is the degree of accuracy which it would be necessary to aim at to secure perfection under the extremely unfavourable conditions we have imposed above. No such state of things would, of course, be possible, even were the elements chosen from the large corrected plate at random; as the thickness varies in a gently undulating manner, and one would never get half the plates equally thick and half equally thin. As a matter of fact, the elements are most carefully selected and grouped according to the final tests of the plate, with the object of avoiding any tendency to such an arrangement as described above.

The testing is carried out by the use of the arrangement generally known, I believe, as Lummer's method. The apparatus is shown in diagram in Fig. 2. Light (which must be a homogeneous radiation) from the source S is roughly collimated by the lens L. A portion of it is reflected from the lightly silvered glass M normally on to the plate E, which is destined to be cut up to form the plates of the echelon. Reflection occurs at both the top and bottom surfaces of this plate, and the portion of the reflected light which passes through the mirror M is observed by the telescope T. Interference rings are formed, which, as the plate E is moved about parallel to itself, open out or close in according as the part under observation becomes thicker or thinner.

It will be readily seen that if one ring opens out and is replaced by another of the same size, that the relative retardation of the two interfering beams (from the top and the bottom of the plate) has increased by λ, λ being the wave-length of the radiation from the light source.

Now the relative retardation of these two beams is $2\mu t$; so that the change of thickness corresponding to one ring is $\dfrac{\lambda}{2\mu} = \dfrac{\lambda}{3 \cdot 16}$ for the light flint of $\mu = 1 \cdot 58$ which we are in the habit of using.

As one can detect about $\frac{1}{20}$th of a ring, it will be seen

Fig. 2.

that one can detect a difference of thickness of $\dfrac{\lambda}{62 \cdot 6}$. This would enable one—putting other difficulties on one side—to construct an echelon of 54 plates, which would perform practically perfectly, even in the extremely unlikely event of the first half of the plates being equally thick, and the second half equally thin, and all on the extreme limit of the undetectable error; while it would be extremely probable that with judicious arrangement of the plates, a 100-plate echelon made to this accuracy would perform perfectly. The production of a plate large enough for a 100-plate echelon to such an accuracy would, however, be a feat of patience and endurance for all concerned, from the glass-maker to the mounter. Yet with unlimited time and undivided attention I believe it would be just within the bounds of what is possible.

We have up to the present ignored all sources of error save those arising from imperfections of the original plate. There are, however, several possible sources of error in the thicknesses of the final plates which occur to one. It will perhaps be of interest to consider some of these.
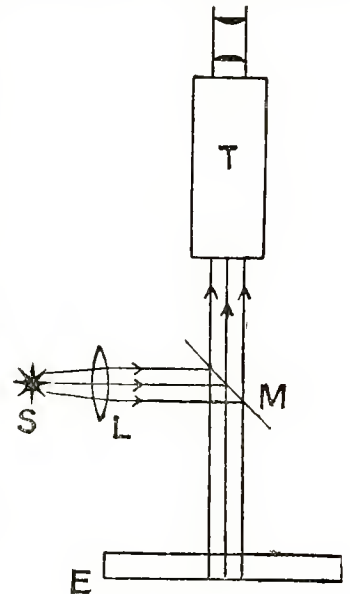
(*A*) Is it possible to obtain plates of glass sufficiently homogeneous?

The plates used are very beautiful plates of light flint, annealed with great care by Messrs Chance Bros. & Co., and I should like to take this opportunity of making known that out of about twenty such plates that we have used for the purpose, we have had not a single failure. It will be noted that the method of testing is such that the *equivalent* thickness is made uniform. Thus for the radiation for which the plate is tested it would not matter even if the plate were of non-homogeneous refractive index within fairly wide limits. But it will be seen that in such a case the equivalent thickness would be uniform for this radiation alone, as the variations in refractive index would be accompanied by slight variations in dispersion. Experience shows that want of homogeneity sufficient to cause perceptive detriment does not exist in the plates used. Our more recent echelons, constructed, as all of them have been, by use of the green radiation (mercury, W.L. 5460), perform quite perfectly on the cadmium red line (W.L. 6438), this being chosen as a test on account of its being a very homogeneous one. I believe it has not been resolved by any means yet brought to bear on it.

We have, I might mention, also a direct check on the homogeneity of the plate.

The thickness of the plates can be measured by mechanical means in the initial stages of polishing to an accuracy of $\frac{1}{4000}$ mm., that is, to $\frac{1}{40000}$ of the thickness, and it has been found, in the case of some plates which were tried in this way, that the mechanical and optical measurements agree to within this degree of accuracy, which represents 4 in the fifth place of decimals in the refractive index.

(*B*) Supposing the plate to have been corrected to the required accuracy, will the cutting of it up cause distortion? The answer to this is; under certain circumstances, yes; but with due care, no. For instance, the plates must not, of course, be cut up with a diamond. Neither, in using the slitting wheel, must too great speed or force be used. As a matter of fact, we always use the old-fashioned hand saw fed with emery, that being the safest; and with this we find that neither on the proof plane nor by the interference test described above can any distortion be, as a rule, detected.

(*C*) A third point worth mentioning is this. The edges of the plates are ground to a very fine matt surface. Now for some reason the region near a matt surface is in a state of strain. It seems probable that this is due to the grinding material which, in crushing pieces out of the glass surface, subjects the part near the surface to a permanent strain from which it does not completely recover. This is easily detected by the use of polarised light, in the case of small pieces, say 1 mm. thick. With the case of fairly massive pieces of glass, however, which do not permit the strain to be transferred inward by the bending of the whole piece, the strain becomes confined to an extremely thin skin of glass, and does not seem to have the slightest deleterious effect on the action of the echelon.

A strain such as mentioned above is entirely removed by polishing, which shows that it originates extremely near the surface.

One may reasonably suppose, in fact, that as the thickness removed by polishing in a particular case was within about $\frac{1}{5000}$ of an inch, that this is the thickness in which the strain originates, and an approximate calculation seems to indicate that the strain is *of the order* of the crushing strain of glass. If the glass is thin, say 1 or 2 mm., this pressure over the skin bends the glass as a whole, and consequently is transferred inwards to a considerable depth; but if the glass is thick it does not bend by an appreciable amount and the strain is extremely local.

(*D*) The fourth point is this :—

In the echelons as now manufactured by Adam Hilger, Limited, we clamp the plates together by means of rods of nickel steel of the same expansion as glass—this substance being chosen to avoid a change of clamping pressure due to change of temperature.

The advantage of this clamping is, first of all, security and convenience of handling; secondly (and this is the more important point), the plates being very carefully cleaned, are pressed into optical contact over the major part of their surfaces, with the result that, instead of the light which traverses a number of plates being considerably decreased in intensity by numerous reflections, the only loss of light is that due to absorption of the glass, which is for the visible part of the spectrum very little

indeed, so that although it is necessary to use a great deal more glass, the gain in efficiency well repays the extra labour involved. Now we may ask, What is the effect of this clamping?

This is best seen by taking an echelon, and commencing with it quite free, gradually clamping it. We will suppose that the radiation observed is sensibly homogeneous. One sees then a sharp line image of the slit. On subjecting the plates to more and more pressure one gets then successively :—

First. No perceptible effect.

Second. An alteration of focus.

Third. Increased alteration of focus accompanied by impaired definition.

The alteration of focus seems to be due to the same cause as the curvature produced on a beam of light originally collimated when it undergoes diffraction from a plane ruled grating, the lines of which get uniformly closer from one end to the other; for obviously the pressure is greater on the smaller plates of the echelon, and decreases uniformly towards the larger plates. Hence it seems reasonable to suppose that the retardations caused by the successive plates will also alter uniformly from one end to the other.

The important point for our consideration at present is, however, that the alteration of focus occurs considerably before any loss of definition. Thus if the clamping of the plates is continued until an alteration of focus becomes perceptible, and is then slightly released, we may feel secure that no deleterious influence is exerted on the performance of the echelon.

I should now like to mention a form of spectroscope which, although its complete theory is more complicated than that of the echelon, is in its actual parts simpler, though the requisite plates are nearly as difficult to construct to perfection; I refer to the parallel plate device of Dr Lummer. I understand that Dr Lummer exhibited one of these at the British Association last year; but as that was, I believe, the only time it has been shown in this country, we have set up in Room 92 an arrangement of this kind for the benefit of those to whom it is new. For purposes of comparison a 20-plate echelon has been erected alongside which also shows the green radiation of mercury and its components.

The arrangement, which was described by Dr Lummer in the *Annalen der Physik, Vierte Folge, Band* 10, 1903, is shown in Fig. 3.

The light from the source S, after passing through the collimator C, falls on a piece of plane parallel glass of very great accuracy E, on which is cemented an ordinary right-angled prism R, as shown in the figure. By means of this prism light is enabled to enter
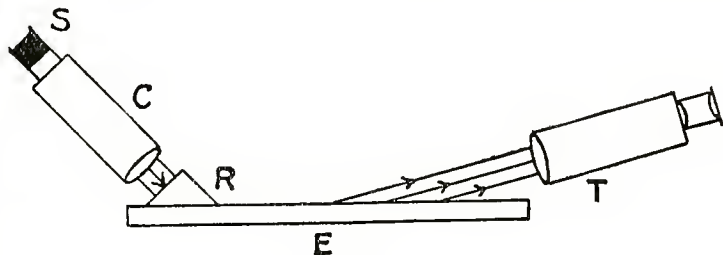
Fig. 3.

the plate at such an angle that its reflection from the internal face of the plate is at almost (but not quite) the angle of total reflection. With the light which passes through the plate to the side remote from the collimator we will not concern ourselves, although Dr Lummer has worked out the interference phenomena exhibited by it in the paper referred to above.

If we trace the reflected beam, then, we note that it strikes the first surface of the plate (that is, the surface nearest to the collimator) at almost the total reflection angle. Thus a small part of the light will be refracted out of the plate at a very high angle of refraction, and a large portion will be again reflected on to the second surface.

It is evident that the beam of light will be reflected backwards and forwards between the two surfaces of the plate, the angle of incidence being always the same, and at each reflection at the first surface a small part of the light will emerge.

If r is the angle of incidence of the light on the second surface, and i the angle of refraction out

of the plate from the first surface, t the thickness of the plate, and μ the refractive index for a ray of wave-length λ, then each emergent beam is retarded behind its predecessor by the equivalent distance $2\mu t \cos r$, and spectra are formed by interference in much the same way as in the case of the echelon.

A number of successive orders of spectra are visible—the intensity of each one being less than in the case of the echelon; but only a complete analysis will enable a correct comparison of the relative intensities to be made, as with Lummer's arrangement *an open slit* is used. A homogeneous radiation would give a number of "Interference maxima," between which are seen—if present—any component radiations. The rate of falling off of intensity from the centres of these interference maxima depends, of course, on the perfection of the plates used; but in the case of accurately worked plates, and in the case when the light emerges at almost grazing emergence, one can, according to a particular case mentioned by Dr Lummer, see as distinct two radiations separated by about $\frac{1}{10}$th of the distance between two interference maxima. This was with a plate giving about fifteen reflections. In the case of a 1 cm. thick plate, such as we are showing in Room 92, the spectra observed will be of the 40,000th order (about) for yellow light; one should then be able to distinguish as separate two radiations differing by $\frac{1}{400000}$ of the wave-length, *i.e.* the resolving power is 400,000. For purposes of comparison one may mention that a 33-plate echelon, with plates 10 mm. thick, gives a resolving power of 329,000.

Great as is the power of such an arrangement, it seems scarcely likely that it will be generally adopted in place of the echelon. Yet it must be noted that although the plates should be more perfect than those of the echelon (for an equal number of beam elements an accuracy about three or four times as great is requisite), they are scarcely as difficult to make, as only a strip has to be good, whereas in a plate destined for an echelon a large area has to be perfected. Thus there will be no objections to its use on the part of the maker.

It is a very beautiful and a very powerful device for the minute analysis of the spectrum, and a perusal of Dr Lummer's paper mentioned above will well repay any one for the trouble.

In this very brief and imperfect description of the apparatus, I have been obliged to omit even mentioning many of the most important and interesting points in its theory. Perhaps an examination of the apparatus itself will bring some of these points up; so that I hope that any one who may be specially interested in it will manage to find time to see it some time later on in the day in Room 92. I shall be very grateful for any advice, hints, or criticisms on this paper. As an instance of the value of such advice I should like to recall that when the late Mr Otto Hilger undertook the construction of the first echelon made in this country—an undertaking made possible by the generosity and patriotism of Lord Blythswood in ordering one—Sir George Gabriel Stokes afforded another instance of his well-known disinterested kindness by writing many and painstaking letters of advice on the subject, without which it is probable that the completion of the project might have been indefinitely postponed.

CONSTANT DEVIATION SPECTROSCOPES.

By Thomas H. Blakesley, M.A.

The great purity of the spectrum produced by these instruments, as made by Messrs Hilger & Co., and their extreme simplicity in use, has caused me to investigate the general question of employing the three sides of a triangular prism in spectrum formation, with a view to finding out the limits of their employment, and the extent to which the dispersion may be carried.

The main fact to be borne in mind is, that each ray should, upon coming into the position of greatest purity and definition, at the same time have a fixed total deviation; so that the collimator and telescope may be once for all fixed in position, and any necessary motion be given entirely to the prism.

The pure condition alluded to is attained in ordinary spectroscopes when minimum deviation takes place.   This phrase loses its meaning in constant deviation instruments, and would be better replaced by that of isogonal or perhaps symmetric passage or refraction, which is really the advantageous accompaniment of minimum deviation in the ordinary case, and gives it its merit.

With constant deviation prisms the condition is one, not of minimum deviation, but of minimum speed of motion across the field of view, the prism being supposed to have uniform motion.

If a ray of intermediate refrangibility, which may be called green, passes into a refracting substance at an angle with a plane surface, it will be accompanied after the passage by rays of less refrangibility which we can call red, diverging from it towards one side, and by rays of greater refrangibility, violet, diverging from it towards the other side (Fig. 1).   To allow the passage out of the medium to take place by a plane surface parallel to the first would only re-combine the rays in direction.   But if the rays are first reflected in such a way that the red and violet rays interchange sides, a plane may be presented to the rays in such a position that the green ray makes the same angle with it as it did with the first plane on leaving it.   The rays will then pass through with increased dispersion, and the green ray will have the quality of purity, produced in the ordinary use of a prism

Fig 1.

at minimum deviation.   The reflecting surface employed must be perpendicular to the plane of passage, but otherwise may make any angle with the green ray consistent with the conditions of total reflection. In the reflection, the deviation which has taken place in the green ray is twice the angle which the ray makes with the reflecting surface.   The second refracting surface must therefore, in order to maintain the isogonal condition, be turned from parallelism with the first refracting surface by this angle.   If, therefore, the refracting surfaces be produced they will meet in this angle, which is called γ.   The reflecting surface will also meet the refracting surfaces in angles β and α respectively, the whole forming a triangular prism in which the three faces are employed either in refraction or reflection.   After passing through the second refracting surface the green ray will make the same angle with this surface as the original ray from the collimator makes with the first surface.   Hence the outcoming ray makes the same angle with the ingoing ray as the two refracting surfaces make with one another, that is γ.

Hence the first rule with such instruments is, that the angle of deviation, which is the angle that the collimation lines of telescope and collimator make with one another, must be equal to the angle opposite the side used as a reflector, and the angle between the reflecting side and the ray is half this angle *i.e.* $\frac{1}{2}\gamma$.

Any ray which follows this course inside the glass will have the quality of purity, and any ray may be made to do so by merely changing the incidence, *i.e.* by turning the prism.   The angle of refraction at the first refracting surface being constant (equal, in fact, to $\frac{1}{2}(\beta - \alpha)$)[1] it follows that the angle of Incidence I is such that

$$\text{Sin I} = \mu \sin \tfrac{1}{2}(\beta - \alpha),$$

and as I represents the position of the prism measured from some zero, it follows that, measured from

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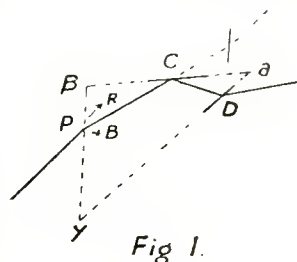
[1] To prove the above value for the angle of refraction $\frac{1}{2}(\beta - \alpha)$, call this angle θ.
   Then $\pi/2 - \theta = \pi - \beta - \gamma/2$
   and $\pi/2 - \gamma/2 = \alpha/2 + \beta/2$.
      $\therefore \beta - \theta = \alpha/2 + \beta/2$, and hence
         $\theta = \frac{1}{2}(\beta - \alpha)$.
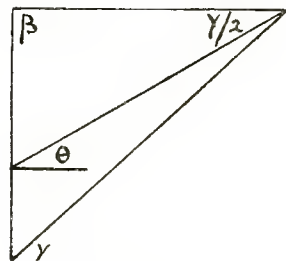
Fig. 2.

this zero the sine of "the angle of position of the prism" is always proportional to the index of the ray, which is at the centre of the field of view of the properly placed telescope.

In the ordinary use of the prism at minimum deviation, the angle of refraction is equal to half the refracting angle of the prism, therefore the angle $(\beta - a)$ is the equivalent angle of refraction of the prism.

The angle of incidence on the reflecting side of the prism is $\pi/2 - \gamma/2$ and is the angle which must exceed the critical angle for total internal reflection. The sine of the critical angle is $\dfrac{1}{\mu}$.

$$\therefore \quad \text{Sin } (\pi/2 - \gamma/2) \text{ or Cos } \gamma/2 \text{ must be greater than } 1/\mu$$

It thus appears that generally any triangular prism can be employed as a constant deviation instrument.

If the angles in ascending order of magnitude are ABC the largest dispersion would be given by making B the angle between the telescope and collimator, for then $\overline{C - A}$ is largest.

An equilateral triangle is the only form which, under no circumstances, will give a spectrum, for the difference between any two angles vanishes.

The denser the glass the greater may the angle opposite the reflecting side be made, and therefore the further the telescope and collimator be from the condition of coincidence in their lines of collimation, and the more likely the possibility of employing the difference between the greatest and smallest angle as the equivalent refracting angle.

Having fixed upon the angles to be employed it does not follow that the whole triangle is to be employed, in fact generally there will be an unutilised portion which can be taken away by truncating the angles.
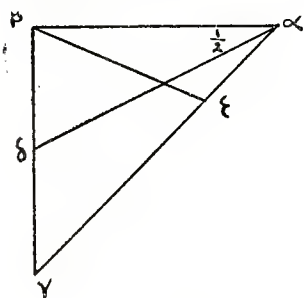
Fig. 3.

Suppose $\beta > a > \gamma/2$.

From the point a (Fig. 2) set off the angle $\beta a \delta$ equal to $\gamma/2$, the point δ being situated between β and γ.

It is clear that the portion $\gamma \delta$ of the first refracting surface is useless.

From the point β set off the angle $a \beta \epsilon$, equal to $\gamma/2$, ϵ being situated between γ and a.

The portion $\gamma \epsilon$ of the second refracting surface is useless.

Hence the angle γ can be truncated by a plane passing through $\delta \epsilon$.

It can be proved that $\overline{\beta \delta} = \overline{a \epsilon} = \dfrac{\overline{a\beta} \, \sin \dfrac{\gamma}{2}}{\cos \dfrac{\beta - a}{2}}$

If $\beta > a$ and $a = \gamma/2$, then δ would coincide with γ and the whole of the first surface would be useful, and $\overline{a\epsilon}$, the useful portion of the second refracting surface, is equal to $\overline{\beta\gamma}$. No truncation should take place as the whole of the triangle is traversed by light.

If $\beta > \gamma/2 > a$,

From γ (Fig. 3) draw $\overline{\gamma\zeta}$ making an angle $\gamma\zeta\beta$ equal to $\gamma/2$ and cutting $\overline{\beta a}$ in ζ, and from ζ draw $\zeta\epsilon$, making the angle $a\zeta\epsilon$ equal to $\gamma/2$ and cutting γa in ϵ.

Then $\overline{a\zeta}$ and $\overline{a\epsilon}$ are useless portions of the two faces $\overline{a\beta}$ and $\overline{a\gamma}$, and a may be truncated by the plane passing through $\zeta\epsilon$.

In this case again $\overline{\beta\gamma} = \overline{\beta\zeta} \dfrac{\sin. \dfrac{\gamma}{2}}{\cos \dfrac{\beta - a}{2}}$

Fig. 4.

and $\overline{a\epsilon} = \overline{a\zeta} \dfrac{\sin \dfrac{\gamma}{2}}{\cos. \dfrac{\beta - a}{2}}$
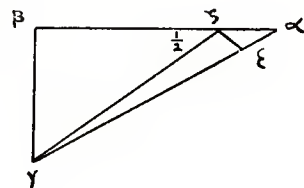
In all cases the area of entrance of light is equal to the area of exit, as of course should be the case from the fundamental idea that the reflecting surface receives from the entrance space, and gives to the exit area, light at the same angle.

The accompanying diagram (Fig. 5) sets forth the cases generally. In it γ and $\overline{\beta-a}$ are represented as co-ordinates on a rectangular system to a certain scale, and β and a are represented by another rectangular system of co-ordinates on a scale compared with the first as $\sqrt{2}:1$ and so combined together that any point represents some triangle on both systems.

On a third set of co-ordinates $\overline{\beta-a}$ is co-ordinated with the index of refraction in such a way as to show the relation corresponding to certain angles of incidence 60° and 75°, with a view to guidance in the selection of glass.
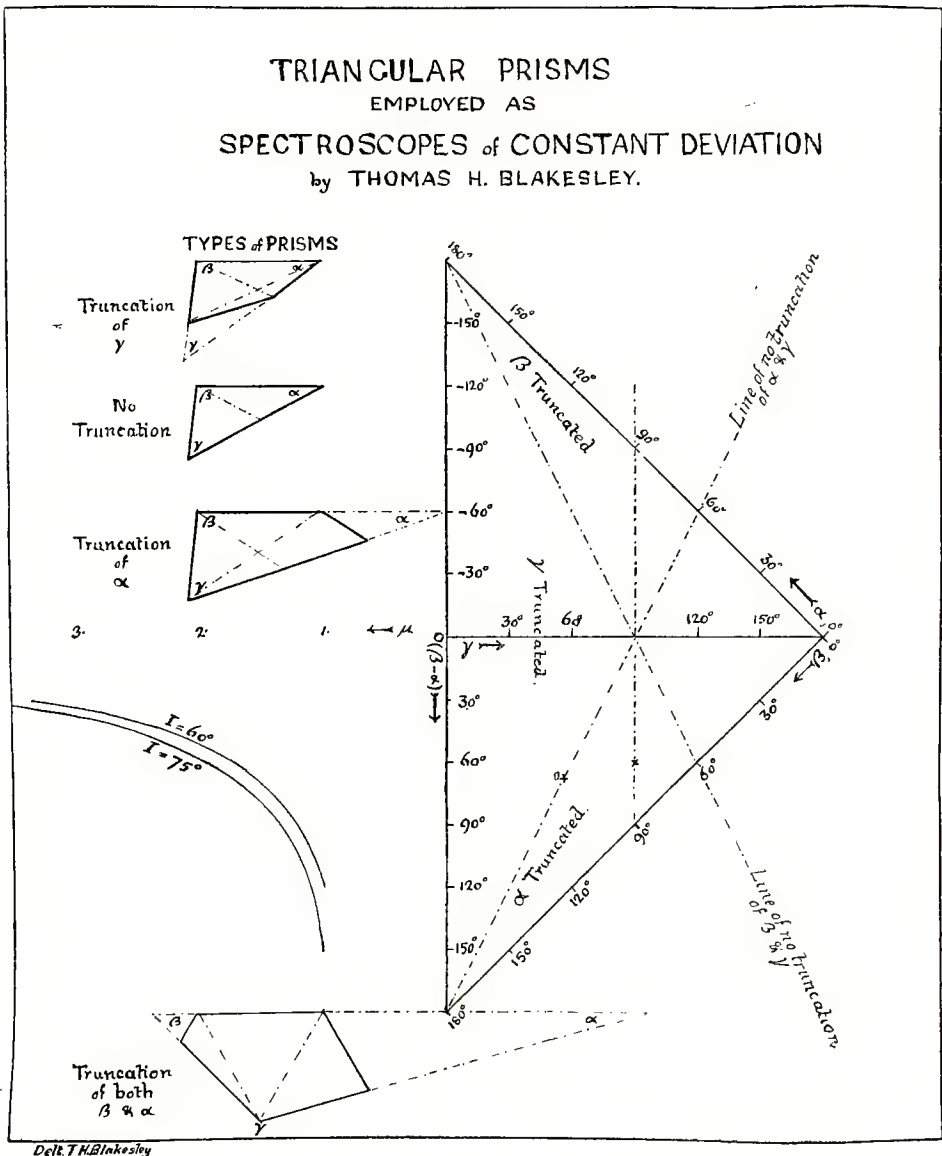
Fig. 5.

Upon the diagram is shown a point which represents Messrs Hilger's form of construction, and the position shows that the angles are $\beta = 75°$, $\alpha = 15°$, $\gamma = 90°$.

The telescope and collimator are therefore at 90° from one another, and the angle of incidence on the reflecting side $\left(\dfrac{\gamma}{2}\right)$ is 45°, the equivalent angle of refraction $\overline{\beta - \alpha}$ is 60°, the angle α is truncated, and this shape will require as large an angle of incidence as 60° for an index value of 1·7.

The same shape with an index of 1·95 would require an angle of incidence of 75°.

The diagram shows the regions where the various angles may be truncated, and the boundaries of these regions are formed of points where no truncation is to be made.

It will be observed that there are regions in which both α and β may be truncated. This occurs when $\dfrac{\gamma}{2}$ is greater than both α and β, and the diagram shows that with very extreme values both of index and incidence such forms might be useful, as in working with extreme ultra-violet rays; γ being in such cases large, such forms could not well be employed on the visible rays.

To obtain the zero of the angle scale for which the angles are such that the sine is equal to the product of the index with the sine of the angle $\frac{1}{2}(\beta - \alpha)$

If I is the incidence,

Sin I $= \mu$ sin $\frac{1}{2}(\beta - \alpha)$.

It is clear that if μ can be imagined equal to unity, the incidence would then be equal to $\dfrac{\beta - \alpha.}{2}$

In that case the ray would pass through the prism as if a mirror were substituted for the prism in the position of its reflecting side.

But the same condition is obtained if the prism is turned 180° from such a position, and the *external* surface of the reflecting side used as a mirror.

Now this is easy of attainment, and if the vernier be read under these conditions it is only necessary to add or subtract 180° to or from the reading to get the position of the vernier when a ray of index unity would pass by reflection and no refraction to the centre of the telescope field.

Hence from this position it is only necessary to turn through $\frac{1}{2}(\beta - \alpha)$ to obtain the reading which corresponds to the zero of the angles of incidence. An approximate idea of the position of this point of the circle may be obtained by remembering that in this position the incidence would be 0°, or the first and second refracting surfaces perpendicular to the collimator and telescope respectively.

In speculating upon the uses of such instruments, it is clear that the single motion necessary to obtain purity in any portion of the spectrum constitutes a very great advantage over forms of spectroscope in which the telescope and the prisms have to be moved separately. In stellar spectroscopy, where no great dispersion is necessary, these instruments would probably be of great service. It is true that with some forms of the zigzag type only one motion is employed, but with two kinds of glass there is always a dispersion which depends in a complex manner upon the indices of both glasses, and such instruments require empirical standardisation for the identification of the light observed. The simple sine law connecting Incidence and Index makes the constant-deviation instruments in such a case greatly preferable.

In photographing ultra-violet portions of a spectrum this simple law would enable the refrangibility of the ray occupying the centre of the field to be fixed with great accuracy. Forms which are not suitable, perhaps, for the less refrangible portions might still be of great use in the ultra-violet, in discovering absorption bands, and their relative positions.

There is also a field for such instruments in colour mixing, which can be carried out in the following simple way :

If a pile of say two, three, or four equal and similar prisms are placed one above the other to a total height suitable to the collimator and telescope, we can with each portion bring to the centre of the field of view what part of the spectrum we please, still retaining purity in each part, with the result that

the corresponding colours are superimposed upon the retina of the eye. Proportions can be adjusted when thought necessary by suitably stopping off part of the thickness of any of the prisms. It is also very convenient to be able in this sort of operation, especially with two prisms, to turn one of the prisms over, so that B and A interchange places, and give a spectrum of colours running the opposite way to those produced by the other prism. The third angle remaining the same, the angles of incidence and refraction remain as before for any colour with the prism turned over.

I have employed two prisms in this way, fixing each upon some line in the solar spectrum with a narrow slit, obscuring the other prism for this adjustment. The slit can then be widened and a splendid chromatic result obtained. At the eye-piece focus a quadrilateral stop can be used for shutting out rays remote from the centre of the field of view.

If the collimator lens employed has been thoroughly corrected for chromatic aberration, an instrument made of one or of two prisms might be found extremely useful in testing telescopes for chromatic aberration at different points of the spectrum singly or at the same time. By the smallest possible tilting of one of the prisms when two are employed, the spectra may be exhibited side by side in great perfection.

Any cause which tends to shift the absorption lines of a spectrum, as the approach or recession of a celestial source of light, may be conveniently investigated by means of two layers of constant deviation prisms, producing spectra running in different directions. If any line of one spectrum is brought to coincidence with its fellow, or indeed with any other line of the second spectrum, for a stationary source of light, any motion actually given by the cause supposed will produce a change of position in *opposite* directions, and the motion observed will be the sum of the absolute motions.

I have to this point confined myself to the use and consideration of one prism of constant deviation, but the placing of such prisms in trains to obtain increased dispersion is a question of great interest, involves no great difficulty, and has some advantages over trains of more ordinary form.

Keeping to prisms of the same shape, and employing them with the same deviation angle γ, there are four possible cases of arrangement.

1. We may employ in a second prism the refracting sides in the same order as in the first prism, or in the reverse order.
2. We may place the second prism upon the side similar to that on which the first prism rests, or we may turn it upside down.

These relations may be described shortly and respectively as similar or inverse order of sides; and as similar or inverse direction of edge.

If in the way suggested by Stanley Jevons we take A and B to represent the condition of the first prism in these respects, and a and b the contraries, then the following will be found to be true in respect of deviation and dispersion produced by the second prism.

If it be employed as ab, both deviation and dispersion are increased.
,, ,, Ab, the deviation is annulled, and the dispersion increased.
,, ,, aB, the dispersion and deviation are both annulled.
,, ,, AB, the dispersion is annulled, and the deviation increased

Hence the dispersion is annulled if the second prism has the same direction of edge as the first, and so far as spectroscopes are concerned we may dismiss from consideration all cases in which the prisms are not alternately employed upon different sides.

Also, as deviation may be considered to be a thing to be avoided, the plan Ab, would probably be preferred to ab except in special circumstances. A train would then be represented by

AB, Ab, AB, Ab, AB, etc.

If the number of the prisms is even there would be no final deviation, and if odd, the deviation would be that due to one prism only.

Suppose such a train to be in adjustment for some particular ray. Then the angular motion to be given to the prisms to bring any other ray into the centre of the field is of the following simple character

The first prism must have exactly the same motion as if it was the only prism in employment. The same angular motion must be given to the third, fifth, seventh, and so on, and exactly the *opposite* angular motion must be given to the second, fourth, sixth, etc. Thus the motion is independent of the number of prisms employed. In an ordinary train the motions may be described as cumulative, the motion of the last prism in a train of eight being much greater than in a train of only three or four prisms. Trains of constant deviation prisms therefore possess the constant deviation property, so that the telescope need never be shifted in angle.

The separate prisms might be mounted upon the alternate bars of a lattice-work lazy-tongs, the extreme points of the centre line of which may be kept upon one straight line.

The angular motion would then be rigorously correct for every prism, and would be of the order 5° between the Fraunhofer lines A and G. This small motion, not being cumulative, is not large enough to cause grave error in respect of the aspect of the successive entrance and exit surfaces to one another.

THE SPECTROSCOPE IN ASTRONOMY.

By H. F. Newall, F.R.S.

(Abstract.)

[This paper, which follows very closely the paper "On the General Design of Spectrographs to be attached to Equatorials of Large Aperture, considered chiefly from the point of view of Tremor-discs, by H. F. Newall," in the *Monthly Notices of the Royal Astronomical Society*, April 1905, is printed in abstract only.]

In spectroscopy of the stars the necessity of using wide slits and high resolving power has been recognised for some time past, but it is of interest to obtain a numerical estimate of the dimensions for which the absorption in the spectrograph counterbalances the light gathering power of the equatorial, the spectrograph being so designed that its resolving power is proportional to the aperture of the equatorial to which it is attached.

The problem of obtaining the spectra of faint stars has been attacked by attaching spectrographs to existing equatorials, and the question of the intensity of light on the slit has been unduly emphasised; but complete installations, including equatorial and spectrograph together, are now being designed, among others by Vogel and Campbell.

The problem to be solved is best shown by the comparison of a large and a small installation.

With a $31\frac{1}{2}$ in. equatorial, as compared with a 12 in., more light will be collected in the ratio of 64 : 9. But because of the greater thickness of the larger object-glass, the light transmitted will be about 47 per cent. instead of 64 per cent. Hence, so far as light transmitted by the object-glasses is concerned, the ratio is 47 : 9 instead of 64 : 9. The image of a star on the slit of the spectrograph is larger with the larger object-glass; and accordingly, unless the slit is made wider, a smaller percentage of the light in the star image is transmitted; but if the slit is made wider, the prisms must be increased in size or number, or the standard of purity and perfection in the photograph lowered. But the standard of the photograph is the lowest that can be advantageously used; hence larger prisms, and consequently greater absorption, are necessary. The width of the slit can be increased until this increased absorption balances the increased transmission at the slit.

The subject is thus of special interest to the Optical Convention, since it emphasises the desirability

of paying attention to the transparency of new glasses quite as much as to their refractive properties. It would be in accordance with modern requirements if the makers and vendors of optical glass would furnish, with all samples of glass, the coefficients of transmission for three points in the spectrum, at say, wave-lengths 6000, 5000, and 4000, for a thickness of 10 cm. of glass.

DIFFRACTIONAL METHOD OF ESTIMATING SLIT WIDTHS.

In this discussion, and in all spectroscopic work, it is most convenient to estimate the slit width in terms of visible diffractional phenomena. The slit is turned towards a distant source of light, the diffraction bands due to the slit are observed on the object-glass of the collimator as on a screen, and the slit may be adjusted to any desired width by observing the diffraction pattern and noting the number of diffraction bands which fall on the objective. If the object-glass has an aperture a and a focal length f, the slit width is given by

$$s = \frac{2m\lambda f}{a}$$

when the mth bands fall on the edges of the object-glass, and m is called the diffractional indicator. The relation between the maxima in the star's image which fall on the slit and the diffractional indicator of the collimator is easily found. When the aperture ratios of the equatorial and the collimator are equal, as is usual in stellar spectroscopy, the slit will transmit m maxima in the diffractional pattern of the star's image,[1] while the geometrical image of the slit in the focal plane of the camera includes m maxima of the diffractional pattern appropriate to the camera.

In practice m is obtained by observing the diffraction bands on the camera objective when the slit is illuminated by parallel light, the eye being at the focal plane of the camera.

The purity of the spectrum depends on the width of the slit used. If P represents purity of the spectrum, R the theoretical resolving power of the spectroscope, and ψ the ratio a/f, the relation connecting purity, resolving power, and wave-length is, according to Rayleigh and Schuster,

$$P = \frac{\lambda}{s\psi + \lambda} R$$

thus :
$$P = \frac{1}{2m + 1} R$$

Or if Wadsworth's relation is adopted as being nearer to observed conditions—

$$P = \frac{\lambda}{s\psi \cdot \frac{2s\psi - \lambda}{2s\psi + \lambda} + \lambda} R = \frac{1}{2m \cdot \frac{4m - 1}{4m + 1} + 1} R$$

giving for various values of m the following numerical relations between P and R :—

m.	P (Schuster).	P (Wadsworth).		
1	$\frac{1}{3}$ R	$\frac{5}{11}$ R, say	$\frac{1}{2}$ R
2	$\frac{1}{5}$ R	$\frac{9}{37}$ R	,,	$\frac{1}{4}$ R
3	$\frac{1}{7}$ R	$\frac{13}{79}$ R	,,	$\frac{1}{6}$ R
4	$\frac{1}{9}$ R	$\frac{17}{137}$ R	,,	$\frac{1}{8}$ R
5	$\frac{1}{11}$ R	$\frac{21}{211}$ R	,,	$\frac{1}{10}$ R

Thus by mere inspection of the diffraction pattern on the object-glass of the collimator the fraction of the theoretical resolving power actually used can be estimated.

[1] For the complete proof of this statement and its limitations see H. F. Newall *loc. cit.*

A series of observations was made to determine the loss of light due to the diffractional spread in the collimator, and the results show that the loss is just perceptible when $m = 4$, and is about 37 per cent. when $m = 1$; and the practical deduction is made that m should never be less than 3, when economy of light is important. The use of so wide a slit as that indicated by $m = 3$ is rendered possible in stellar work by the fact that the slit is illuminated by a tremor-disc of considerable dimensions.

Experience shows that it is only under exceptional circumstances that stellar photographs exhibit star-images of smaller diameter than 1″; and probably most observers would be content if the smallest well-formed star-images on their photographs did not exceed 3″ This experience has been gained with refractors and reflectors, large and small; and there is a general belief that the atmospheric tremor is mainly responsible for the fact that smaller images are not usually attainable. We may take it, therefore, that the smallest effective accumulative image of a star on the slit of a star-spectroscope is a " tremor-disc" of diameter 2″ or 3″ at least.

" The name [" tremor-disc "] more or less explains itself; it is easiest to state what it is intended to convey by reference to a photograph of a star taken with a long exposure. The star image [diffraction pattern] moves about on the plate in consequence of atmospheric tremor, and produces its effect at each spot on which it rests; the developed image is strongest where the star has most frequently rested. The distribution of density is probably symmetrical about the mean position of the star, and the intensity at different points along a diameter of the resulting tremor-disc is probably fairly well represented by a 'law of errors' curve. Apart from the photograph which shows the summation of effects the tremor-disc may be conceived as existing in time, so to speak; and the effect produced in a slit-spectroscope depends on the relation between a certain area of tremor-disc and the area of the slit illumined by it. The tremor-disc is of greater importance, so far as the design of a stellar spectroscope is concerned, than the diffraction-disc, which has generally been considered" (*Monthly Notices* [1896], vol. lvi. p. 108).

For the purpose of numerical estimates the effects of atmospheric disturbances may be regarded as (1) Scattering of light over a very considerable field (say 40″ to 50″), and (2) of movements of the main concentration of light through small distances (say 5″) from the mean position; the former light is completely lost while in the tremor-disc is summed up the light which forms the image proper.

The percentage of light scattered may be assumed proportional to the diameter of the object-glass, while the dimensions of the tremor-disc are independent of the aperture, and may vary from hour to hour. For numerical estimates it is assumed that the distribution of light may be represented with sufficient accuracy by a truncated cone, where the diameter of the " core" is represented by the top of the cone, while the base represents the tremor-disc. Stellar photographs prove that the core is seldom less than 2″ in diameter.

The following table shows what fraction of the total quantity of light in a tremor-disc of diameter τ'' is collected in the core of diameter γ'':

TABLE I.

$\tau'' =$ 2	3	4	5	6	7	8	9	10
$\gamma = 1$ ·43	·23	·14	·10	·07				
$\gamma = 2$	·63	·43	·38	·23	·18	·14	·12	·10

When the slit is wide enough to transmit the whole of the core, Fig. 1 indicates the " transmission " which can be easily evaluated, the " transmission " being the ratio of the transmitted light to the total.
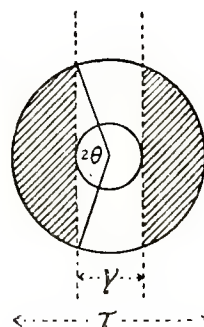
Fig. 1. Tremor-disc and core on a wide slit.

The "transmission" for narrower slits, as well as "core-wide" slits, are tabulated :—

TABLE II.

Transmission at a Slit narrower than the Core.

τ″	γ=1″ Slit=γ×.1	.2	.3	.4	.5	.6	.7	.8	.9	1·0	γ=2″ Slit=γ×.1	.2	.3	.4	.5	.6	.7	.8	.9	1·0	τ″
2	·08	·16	·24	·32	·40	·47	·55	·62	·68	·73											
3	·06	·12	·18	·23	·29	·35	·40	·45	·50	·55	·10	·20	·30	·40	·49	·58	·66	·74	·81	·87	3
4	·05	·09	·13	·18	·22	·27	·31	·35	·39	·43	·08	·16	·24	·32	·40	·47	·55	·62	·68	·73	4
5	·04	·07	·11	·15	·18	·22	·25	·29	·32	·35	·07	·14	·21	·27	·34	·40	·46	·52	·58	·63	5
6											·06	·12	·18	·23	·29	·35	·40	·45	·50	·55	6
7											·05	·10	·15	·21	·25	·30	·35	·39	·44	·48	7
8											·05	·09	·13	·18	·22	·27	·31	·35	·39	·43	8
9											·04	·08	·12	·16	·20	·24	·28	·32	·36	·39	9
10											·04	·07	·11	·15	·18	·22	·25	·29	·32	·35	10

Thus a slit which has a width equal to $\frac{4}{10}$ of the diameter of a core $\gamma = 2''$ transmits **27** per cent. of the light incident upon it if the tremor-disc has a diameter 5″.

Considerations relating to the Proper Apportionment of the Resolving Power to the Aperture.

The slit width being given by

$$s = \frac{2m\lambda f}{a}$$

where $m \nless 4$ for minimising diffractional losses, we have in the case of a "core-wide slit of γ",

$$\frac{F\gamma}{2 \times 10^5} = s = \frac{2m\gamma\lambda f}{a}$$

where m_γ is the diffractional indicator for a core-wide slit ; then, since the ratio A/F for the equatorial is the same as that of the collimator a/f,

$$A = \frac{2m_\gamma\lambda \times 2 \times 10^5}{\gamma} \text{ for core-wide slits,}$$

a relation which gives the aperture of the equatorial suitable for economic and efficient work in terms of the wave-length of light and m and γ.

TABLE III.

$$A = \frac{17 \cdot 36 m_\gamma}{\gamma} \text{ for } H_\gamma \text{ and core-wide slit.} \quad \text{(A in cms.)}$$

A Cms.	A Inches.	m_γ for $\gamma = 1''$.	m_γ for $\gamma = 2''$.	A Cms.	A Inches.	m_γ for $\gamma = 1''$.	m_γ for $\gamma = 2''$.
30	11·8	1·7	3·4	91	35·9	5·2	10·4
63	24·8	3·6	7·2	102	40·2	5·9	11·8
80	31·5	4·6	9·2	150	59·1	8·6	17·2

Thus the use of a large aperture A carries with it the disadvantage of a correspondingly large m_γ, and the necessity for a larger resolving power to obtain the necessary purity.

Requirements in Photographed Spectra.—"It seems to me not amiss to start with the following statement of requirements in a photographed stellar spectrum :—

"(1) For the proper identification of lines, a purity of spectrum $P = \lambda/\delta\lambda$ of at least 10,000 is needed, allowing of distinction between lines for which $\delta\lambda = 0\cdot4$ at $\lambda4000$, and $0\cdot5$ at $0\lambda5000$. Thus $P = 10,000$.

"(2) For the proper measurement of wave-lengths, a linear dispersion of at least 1 mm. per 10 tenth-metres is needed. Thus

$$\frac{ds}{d\lambda} = 10^6.$$

"(3) For the proper discrimination between real stellar lines and faults due to defects in emulsion, a height of spectrum (= length of lines) of at least $0^{mm}\cdot25$ is needed.

$$h' = 0^{mm}\cdot25.$$

"With respect to (1) we have seen that the purity of the spectrum is given by the relation $P = R (2m + 1)$, m being the diffractional indicator for the slit used, and R the resolving power of the spectrograph. If we are to utilise the greater part of the light in a tremor-disc, we must have $P = R(/2m_\gamma + 1)$. Thus Table IV. tells us that for a 25-in. object-glass R must be 154,000 to give $P = 10,000$ with a core-wide slit.

"With respect to (2), viz. the linear dispersion, bearing in mind that $R = ad\theta/d\lambda$, where a is the linear aperture of the collimator, and $d\theta/d\lambda$ is the angular dispersion produced by the prism-system, we see that the linear dispersion is

$$\frac{ds}{d\lambda} = f_{cam}\cdot\frac{d\theta}{d\lambda} = f_{cam}\cdot\frac{R}{a} = \frac{R}{\beta} = \frac{(2m+1)P}{\beta}$$

and the angular aperture of camera is

$$\beta = (2m+1)P/\frac{ds}{d\lambda}$$

We must arrange that β has a practicable magnitude.

"With respect to (3), viz. the height of spectrum and the duration of trail of the star on the slit needed to give the required height h', we can deal with the question, for the moment, in terms of an undispersed image of the slit on the photographic plate in the camera. If the slit is illuminated by a tremor-disc whose core is γ'' in diameter, then the height of the slit illuminated is $\dfrac{F\gamma}{2\cdot10^5}$, where F is the focal length of the equatorial; and the height of the image of the slit as photographed in the camera is

$$\frac{F\gamma}{2\cdot10^5}\cdot\frac{f_{cam}}{f_{coll}} = \frac{A\gamma\cdot f_{cam}}{2\cdot10^5\cdot a} = \frac{A\gamma}{2\cdot10^5\cdot\beta}$$

Hence if the duration of trail is made to be $h'/\dfrac{A\gamma}{2\cdot10^5\cdot\beta}$. times as long as the time required for a stationary image, we shall get an image of slit of length h' and with intensity nearly the same as that of the stationary image. Supposing that the photographic density is proportional to It, if an image of intensity I rests on the plate for the time t, then in a given time we shall have the intensity with trail to give $h' = \dfrac{A\gamma}{2\cdot10^5\cdot\beta\cdot h'}\times$ the intensity without trail. [The intensity with trail will he somewhat greater, inasmuch as we may suppose the exposure for the stationary image to be just long enough for the core alone to impress the plate, whereas in the trailed image the outlying parts of the tremor-disc above and below the core contribute something to the final density by 'preparing' the plate for the trailing core.]"

Expressions are then obtained for the intensity of the photographic spectrum with "core-wide" slits and also with narrower slits. From these expressions, assuming a tremor-disc of 5" with a core of 2" and the above conditions for the photographed spectra, tables were calculated for the relative intensities of spectra given by various installations.

TABLE IV.

Relative Intensities of Spectra given by Various Installations.

(Spectrum near H_γ.   Purity = 10^4, $\dfrac{ds}{d\lambda} = 10^6$, $h^1 = \frac{1}{4}$ mm.   Prisms of Jena glass O. 102.   Core-wide slit.

Tremor-disc $\tau = 5''$, core $\gamma = 2''$.)

Aperture. Cm. (Inches).	m_γ.	A^2.	O.	S_γ.	Π(O·102).	$\dfrac{2m_\gamma+1}{2m_\gamma}$	Photographic Intensity.	β.	R.
30 (12)	3·4	900	·64	·63	·450	1·15	189	·078	78000
50	5·8	2500	·57	·63	·278	1·09	271	·126	126000
63 (25)	7·2	3969	·53	·63	·208	1·07	294	·154	154000
70	8·1	4900	·50	·63	·170	1·06	278	·172	172000
80 (31½)	9·2	6400	·47	·63	·138	1·05	275	·194	194000
91 (36)	10·4	8281	·44	·63	·108	1·05	260	·218	218000
102 (40)	11·8	10404	·40	·63	·086	1·04	234	·246	246000

"In calculating O from Vogel's table (*Astroph. Jour.*, v. 89) I have allowed for a diversion of a certain percentage of the light out of the tremor-disc into the scatter disc, on the scale of 1 per cent. for every 10 cm. in the aperture. In the last columns of the table I have entered β and R. Under the assumed conditions of purity and linear dispersion, we have $\beta = \dfrac{2m_\gamma+1}{100}$, and R = $\beta \times 10^6$.

In this table A^2 represents the incident light, O the percentage transmitted, $S\gamma$ the slit transmission, Π the tran-mission co-efficient of the prism train.

Table IV., which gives intensities when the full width of the core is used, shows that under these conditions the turning point of advantage to be gained from large apertures is reached at the 25-in. object-glass. The slit must be narrowed to obtain greater prism transmission, and the results for various slit widths are given in table V., and are shown graphically in Fig. 2.

TABLE V.

Relative Intensities of Spectra given by Various Installations.

(Spectrum near H_γ.   Purity = 10^4, $ds/d\lambda = 10^6$, $h' = \frac{1}{4}$ mm.   Prisms of polarising angle and of Jena glass O. 102.   Tremor-disc $\tau = 5''$, core $\gamma = 2''$.)

Aperture Cm. (Inches).	m_γ	$n = ·1$ $Sn_\gamma = ·07$	·2 ·14	·3 ·21	·4 ·27	·5 ·34	·6 ·40	·7 ·46	·8 ·52	·9 ·58	1·0 ·63.
30 (12)	3·4	83	108	134	146	161 B	171	177	186	190 3·1	189
50	5·8										271
63 (25)	7·2	197	266	315	332 C	352 3·6	348	340	335	315	294
70	8·1										278
80 (31½)	9·2	245	335 P	380	388	401 4·6	387	367	335	305	274
91 (36)	10·4										260
102 (40)	11·8	308	401	438 3·5	426 Y	419	376	333	271	263	234

"In the above table n represents the fraction of the core included by the slit, $S_{n\gamma}$ the corresponding slit-transmission, the figures in a horizontal line represent the intensities for the aperture mentioned in the first column. Under the maximum value is given the diffractional indicator, and under another value a letter is entered to show that an installation exists of dimensions and resolving power suited to work at that value. Thus B indicates Bonn, where Professor Küstner has lately installed an instrument

of resolving power about 43,000 ; allowance should be made for diffractional loss in the collimator for all the values of n, except the two highest, for this installation ; and similarly in other cases whenever $nm_\gamma < 3$. C indicates my own installation at Cambridge ; P = the new installation at Potsdam ;

Y = Yerkes Observatory. At Potsdam, Yerkes, and Bonn the prisms are actually made of Jena glass O. 102 ; at Cambridge they are made of lighter and, I believe, more transparent glass.

"The results show that we are very close to the limits of photographic intensity in spectrographic work, and that some existing installations are near the capacity of the equatorials used."

The suggestion is then put forward to use a diffraction grating instead of a prism-train, and the necessary grating transmission to give results equivalent to the Cambridge installation is found to be 14 per cent., using a third order spectrum of a suitable Rowland grating.

"I am convinced," concludes Mr Newall, "that a 30-in. reflector, properly combined with a diffraction grating spectrograph, would give results which would compare favourably with the most powerful existing installations."

Slit-width in terms of decimals of the core of the tremor-disc.

Fig. 2.

THE OPTICS OF THE SPECTROSCOPE.

By Professor SCHUSTER, F.R.S.

I FEEL almost ashamed to bring this subject to your notice in the presence of Lord Rayleigh, as at the best I can only claim to be his apostle in the teaching of the theory of optical instruments. It was he who first gave us the principles on which the resolving powers of spectroscopes ought to be discussed, and to-day I have only little to add to what may be directly deduced from Lord Rayleigh's work. But I should like to emphasise one or two points which might even now be more generally recognised than they are. For simplicity's sake I will confine myself to prisms, which have been chiefly referred to this morning, but most of my remarks apply to both gratings and prisms. The theoretical resolving power of a prism, or a system of prisms, has been shown by Lord Rayleigh to be proportional to the greatest effective thickness of glass traversed, multiplied by a certain factor, which is the rate of change of the refractive index with wave-length. In mathematical language, if t_1 and t_2 are the greatest and smallest lengths of path traversed by the rays in the prisms, and μ is the refractive index,

$(t_1 - t_2)\dfrac{d\mu}{d\lambda}$ is the resolving power. In the case of simple (not compound) prisms, the shortest length

ought to be zero, so that the resolving power depends on the greatest thickness of glass traversed. This result at once shows that there is no justification for the remark which is still frequently made, that the position of minimum deviation presents any advantages as regards definition or purity of spectrum. In

the case of a prism which is cut symmetrically, the position of minimum deviation happens to be that position in which the passage of the extreme ray passes through the largest amount of glass. But this is a peculiarity of the shape of the prism. With a section in which the two sides are unequal, the prism would give its greatest resolving power in a position which is not at all that of minimum deviation.

Now although this subject has been before us for something like twenty or thirty years, owing to the fact that the published accounts of work done seldom state the resolving power used, it is very difficult to say how far the theoretical resolving power has been realised by various observers and in different spectroscopes. It is not sufficient for an author to state the angles and number of the prisms he used, unless he also give the length of their base. Then there is the optical property of the glass to be considered, and this is a matter in which opticians may help the investigator. Every glass manufacturer knows the refractive indices in different parts of the spectrum of the glass he sends out, and this information should always be furnished through the instrument maker to the user of a spectroscope. Every one might, of course, determine it for himself, yet this would mean a repetition of work which might be done once for all.

When the refractive indices are given for a few lines distributed along the spectrum, the dispersion may be deduced from some interpolation formula. Cauchy's equation is still most often used in this country, though a formula given by J. Hartmann[1] seems to fit the observations somewhat better. If μ be the refractive index and μ_0, c, λ_0, are constants, the refractive indices of different kinds of glass are all capable of being represented by means of the equation

$$\mu - \mu_0 = \frac{c}{(\lambda - \lambda_0)^{1 \cdot 2}}$$

The use of the formula is simplified by the fact that λ_0 has nearly the same value for different kinds of the same type of glass. Thus for different kinds of flint, the values of λ_0 lie between 0.19 and 0.21μ' and for different kinds of crown between 0.17μ' and 0.19μ' ($\mu' = 10^{-4}$ cms.). If the connection between refractive index and wave-length is required only for a restricted portion of the spectrum, Hartmann shows that it is sufficient to use the simplified formula

$$\mu - \mu_0 = \frac{c}{\lambda - \lambda_0}$$

and this equation is very useful when it is desired to obtain wave-lengths in spectroscopic observations by interpolation. Hartmann has prepared auxiliary tables which enable every one to determine without much trouble the constants of the more complete equation, and it would be very desirable to have these tables republished in some easily accessible publication.

An interpolation formula having been obtained, the dispersion is found by differentiation. Thus if Cauchy's formula

$$\mu = a + \frac{b}{\lambda^2} + \frac{c}{\lambda^4}$$

is used, it follows that

$$-\frac{d\mu}{d\lambda} = \frac{2b}{\lambda^3} + \frac{4c}{\lambda^5}$$

and from Hartmann's formula it follows that

$$-\frac{d\mu}{d\lambda} = \frac{1 \cdot 2\, c}{(\lambda - \lambda_0)^{2 \cdot 2}}$$

In the following table I have put together the value of $-\dfrac{d\mu}{d\lambda}$ calculated for three different kinds of flint glass (numbered 1, 2, 3, by Hartmann) and for three different wave-lengths, according as Hartmann's formula (A), or Cauchy's formula (B) is used. I have added under (C) the dispersion

[1] *Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. xii., Appendix.

obtained from the simplified equation derived from that of Cauchy by dropping the last term. In this case the constant b was calculated from the values of μ for the hydrogen lines H_α and H_δ.

$$\text{Dispersion} \left(-\frac{d\mu}{d\lambda} \right)$$

(The unit of the wave-length is 10^{-4} cms.)

Flint Glass.	$\lambda = \cdot6563.$	$\lambda = \cdot4862.$	$\lambda = \cdot4102.$
1.	·06878 (A)	·1837 (A)	·3471 (A)
	·06717 (B)	·1854 (B)	·3388 (B)
	·07501 (C)	·1845 (C)	·3073 (C)
2.	·06165 (A)	·1624 (A)	·3030 (A)
	·06009 (B)	·1642 (B)	·2977 (B)
	·06634 (C)	·1632 (C)	·2717 (C)
3.	·05104 (A)	·1318 (A)	·2416 (A)
	·04953 (B)	·1331 (B)	·2383 (B)
	·05386 (C)	·1325 (C)	·2206 (C)

The table shows that Cauchy's formula gives results differing by several per cent. from that of Hartmann. The resolving power increasing very quickly with the diminution of wave-length, an exact knowledge of dispersion is not required, yet for the comparison of different installations, it seems advisable to adopt a good standard of accuracy, or to calculate the dispersion always in the same manner. It may prove to be more convenient for this purpose to use the refractive indices of certain selected groups of lines, each group consisting of two lines which are fairly near together. We may then substitute $(\mu_1 - \mu_2)/(\lambda_2 - \lambda_1)$ for $-\frac{d\mu}{d\lambda}$.

I would suggest the following combinations :—

For the dispersion in the red, Lithium (·6708) and Hydrogen (·6563).

,, ,, green, Mercury (·5461) and Thallium (·5351).

,, ,, blue, Hydrogen (·4341) and Calcium (·4226).

The blue Mercury line (·4359) might be used in place of the Calcium line.

If the glass manufacturer or optician would take the trouble to calculate the constants of Hartmann's formula, it would be most convenient to have $d\mu/d\lambda$ determined by calculation for the wave-lengths ·6, ·5, and ·4. Thus for the three different kinds of glass to which the above formula applies, I find :—

$$-\frac{d\mu}{d\lambda}.$$

Flint Glass.	$\lambda = \cdot6 \times 10^{-4}.$	$\cdot5 \times 10^{-4}.$	$\cdot4 \times 10^{-4}.$
1.	·0909	·1664	·3842
2.	·0812	·1474	·3346
3.	·0669	·1199	·2658

Whatever system is adopted, some uniformity should be aimed at.

Lord Rayleigh's formula gives us the resolving power on the condition that the slit opening is sufficiently narrow; but it is found by experience that, except when sunlight or the light of an electric arc is used, spectroscopists set their slits at a width several times larger than that to which the equations can reasonably be made to apply.

Having at one time been occupied with observations of very weak spectra, this part of the subject attracted my attention very soon after Lord Rayleigh's first publications on resolving powers. I then gave a formula for what I called the purity of the spectrum as depending on slit-width. That formula was perfectly correct, assuming a certain standard of resolution. There is always something arbitrary in the adoption of such a standard, and one of the assumptions which I made was open to objections. I think Mr Wadsworth was right in some of the criticisms he applied to my investigation, and at any rate for wide slits my formula or test was not as accurate as it ought to have been. Wadsworth gave another formula, which has led to the paradoxical result that when you go beyond a certain point, narrowing the slit deteriorates the resolving power. This may be proved by elementary considerations to be wrong, but it seems to have been generally accepted, and has frequently been quoted.

I recently took up the subject again.[1] I now take a certain test for resolution which is the same as that used by Wadsworth, and which has been found consistent with observation. Instead of attempting to reduce the results to a formula, 1 prefer to enter them into a table which by inspection gives all we want.

TABLE.
CONNECTION BETWEEN PURITY AND WIDTH OF SLIT.

I.	II.	III.	IV.	V.	VI.
				Relative Intensities.	
Width of Slit.	Slit-factor.	Distance of Resolution.	Purity Factor.	Intensity 1 for Normal Width.	Intensity 1 for Infinite Width.
o	o	1·0	1·0	o	o
·1	·4	1·002	·998	·406	·100
·2	·8	1·009	·991	·805	·198
·25	1·0	1·014	·986	1·000	·246
·3	1·2	1·021	·980	1·191	·293
·4	1·6	1·038	·964	1·558	·383
·5	2·0	1·060	·943	1·902	·467
·6	2·4	1·089	·918	2·217	·545
·7	2·8	1·124	·889	2·500	·615
·8	3·2	1·168	·856	2·751	·676
·9	3·6	1·221	·819	2·967	·729
1·0	4·0	1·283	·780	3·148	·774
1·2	4·8	1·438	·695	3·415	·839
1·4	5·6	1·624	·616	3·571	·878
1·6	6·4	1·823	·549	3·646	·896
1·8	7·2	2·022	·495	3·670	·902
2·0	8·0	2·221	·450	3·674	·903
3·0	1·20	3·214	·311	3·789	·931

In the table the first two columns refer to the width of the slit. If λ is the wave-length, a the diameter of the collimator lens, and f its focal length, the slit opening is obtained in centimetres by multiplying the numbers in the first column with $\lambda a/f$.

The third column gives the distance of resolution for the different slit-widths, the distance being taken as unity for an indefinitely narrow slit. If we take the reciprocals of these distances, we obtain numbers which indicate what fraction of the resolving power is retained. These reciprocals are entered into the fourth column, under the heading of " Purity Factor." What is required in practice is the knowledge as to how much of purity we must sacrifice in order to gain light. The relative intensities of illumination are therefore entered in the sixth column. Column IV. shows that with a slit-width $\lambda a/4f$ we secure the ideal resolving power within $1\frac{1}{2}$ per cent. Column VI. shows that narrowing the slit beyond this limit would rapidly diminish the illumination without a corresponding advantage in purity. I therefore call this the normal width of slit, and for other slits I call " slit factor," the number indicating their widths in terms of the normal slit.

The meaning of Columns II. and V. will now be clear. The use of the table is simple. If we do not mind a loss of purity of 6 per cent., we may set the slit at 2, and obtain nearly double the amount of light. We may treble the light, retaining about 80 per cent. of the purity, but a total intensity of 3·67 times that obtained with a normal slit is accompanied by a loss of half the resolving power. When this point has been reached, a further widening of the slit leads to a great deterioration of purity without material increase of light. We are led to the general conclusion that spectroscopes intended for terrestrial purposes and for observations in which light is a consideration, should be constructed so as to give about twice the resolving power of that actually aimed at. We may then set the slit so that its factor is about 7·2. In order to compare the units adopted here with those introduced by Mr Newall, it may be mentioned that my slit factor 8 corresponds to the unit value of his m.

One word as to the conditions which give us the greatest amount of light. If you trace the image of a luminous surface through any number of lenses, the brightness of your ultimate image depends only on one thing, and that is the cone of light which leaves the last lens—provided, of course, that the light which falls on the first lens can also pass through all the others. The illumination of the image under these circumstances is directly proportional to what is called the solid angle of this last cone of light. By solid angle is meant the surface cut out by the cone on a sphere of unit radius.

[1] *Astrophysical Journal*, vol. xxi., p. 197 (1905).

All you have to look to, therefore, in constructing an optical instrument which shall give bright images, is that the solid angle of the beam of light leaving your last lens shall be as large as possible. That is the only thing that matters. When we are dealing with eye observations, this solid angle is fixed for us, and we have no command over it. The ultimate image in this case being that formed on the retina, the solid angle in question depends on the aperture of the pupil and the distance of the pupil from the retina. Therefore we must fill our pupil with light in order to secure the maximum amount of light, but we cannot do more. This condition practically fixes the optical construction; it gives us the magnifying power of the telescope if we know the size of the prism, because knowing the width of the beam leaving the prisms and the width of the beam entering the eye, the ratio of these two quantities is equal to the magnifying power.

Here I must interpose a remark which alters to some extent the theoretical conclusion I have just stated. Owing perhaps to imperfections in our eyes, and also to other matters of a physiological nature, it is often advisable to narrow the emergent beam beyond the limit given. Whether this is generally true may depend on different persons, and I think statistics should be obtained with regard to the matter. Certainly, speaking of my own case, I like to contract the beam which falls into my eye to about half the width of the pupil, putting up with the loss of light which will be reduced to one quarter of its full value. Possibly the outside portions of my crystalline lens are bad, and possibly the larger size of the diffraction image on the retina obtained with the narrow beam may be the cause of the difference, but I can see better and with less fatigue by contracting the beam to something like $1\frac{1}{2}$ mm.[1]

The impossibility of increasing the luminosity in telescopic and spectroscopic observations beyond the point reached when the pupil is filled with light has long been known. Yet it is not always borne in mind by those who design new instruments, and only recently a high authority constructing a spectroscope intended for weak sources of light, praised it on the ground that the ratio of the diameter of the lens to the focal length in the telescopic system was exceptionally great. The brightness of the image is, of course, quite independent of this ratio when images are received by the eye. It is otherwise when photographic records are taken. Here the angular aperture of the cone of light forming the final image may be varied within wide limits, and we may gain in brightness by increasing this angular aperture. A limit will, however, be reached owing to the difficulties of optical construction and the necessity of securing a sufficient dispersion apart from resolving power.

The table given above, which shows the connection between resolving power and brightness or image, is constructed on the supposition that the slit is a self-luminous line or narrow band. This may be secured by using a lens to project an image of the source on the slit. In my opinion, no spectroscopic observations should be taken without the use of such a lens. Only with very narrow slits, such as are seldom used except in the case of the solar spectrum, can this lens be dispensed with. In this case the light spreads out laterally by diffraction, so that the whole collimator lens is filled with light, though the incident beam may subtend only a small angle. But though with narrow slits the lens may be dispensed with as far as the resolving power is concerned, it must be retained in order to gain illumination, and if luminosity is a consideration, some attention must be paid to having a sufficient angular aperture for the condensing lens.

If this angular aperture were equal to that of the collimator lens, the effects of diffraction would spread out the pencil in the collimator tube so that only a fraction of the light would fall on the collimator lens. With the slit equal to what I call the normal slit, only about the eighth part of the light would then be utilised. A good part of the light may be recovered if the aperture of the condensing lens is sufficiently increased. To facilitate this, it is convenient to use a collimator having a small angular aperture, so that we may still secure sufficient definition in the image thrown on the slit by the condensing lens, though the aperture of that lens is four or six times as great as that of the collimating lens. It the slit is set at 2, a ratio of four to one in the apertures of the two lenses would be an appropriate one. When the slit is opened as far as 8, the loss of light by diffraction ceases to be

[1] I ought to state that my eyes are slightly astigmatic, and that at the time I was chiefly observing weak spectra, I had not yet felt the necessity of correcting the astigmatism.

of great importance, and the aperture of the condensing lens need not only be slightly in excess of that of the collimator.

In the above discussion I have exclusively devoted my attention to observations with terrestrial sources of light. The conditions for astronomical work, which in some respects are different, have recently been fully discussed by Mr Newall.

In conclusion, I should like to ask instrument-makers to pay a little more attention to the construction of the slit, which always seems to me to be the weakest part of a spectroscope as now delivered by them. Experiments on questions such as those I have discussed to-day often fail on account of the uncertainty of the width of the slit which is used, and the same uncertainty, no doubt, often prevents observers from giving sufficient information in their publications as to the condition under which they have used their instruments. An observer may himself find, as Mr Newall has shown, the width of the slit in terms of the arbitrary scale fixed by the maker on the mechanism which widens and narrows the slit, but that mechanism is generally so defective that you cannot trust the same readings to correspond to the same width of slit. The constant worry of standardising and restandardising an arbitrary scale ultimately leads to the observer completely abandoning all attempts to keep a record of his slit-widths.

Lord Rayleigh said it was not necessary to remark on the masterly way in which the subject had been treated by both authors; certainly it was a pleasure to him to have thrown out ideas and to see them ultimately developed and utilised in the way they had been. He emphasised the necessity for the utilisation of the aperture of a prism or grating when the light falls upon it in the ordinary way through a slit. In order to obtain the full resolving power of the grating or prism it was necessary that all the width should be utilised, but that was not enough. It would be no use to utilise half the width, for example, with light which comes from one source, and the other half with light which comes from another source, with no phase relation with the first. If we allowed the light from the sun to shine directly through a slit upon a prism very likely the whole width of the prism would not be shone upon, taking into account the angular magnitude of the sun, the distance of the prism from the slit, and so on. It might be thought that the difficulty could be got over and the whole width of the prism effectively used if we interpolated a telescope behind the slit, which had the effect of increasing the apparent angular diameter of the sun. Of course as far as mere accumulating light goes the object would be attained, but in respect of resolving power it would not be attained. The light which comes from one part of the solar disc could not help by interference light which comes from another part, and consequently you could not by that means expect to render available the whole width if it would not have been available without the use of that device. This touched upon some of the more delicate questions of optics. There was no difference in the conclusion, to which we should be led from that point of view, from that put forward by Professor Schuster and Mr Newall. In order to obtain the maximum of resolving power the whole width of the prism should be utilised by diffraction. If the light coming from the slit operated by diffraction, so as to cover the whole face of the prism or grating, then the required phase relationship obtained, and ordinary theoretical conclusions follow. His own experience of spectroscopes was rather at the other end of the scale from that with which Mr Newall dealt. He had worked on the sun and not at all upon the stars, so that the difficulties he had had to encounter were very different from those discussed by Mr Newall. He had sometimes utilised the diffraction fringe to show whether the slit was of the right magnitude to obtain the necessary purity, but had never attempted to count the diffraction bands. The method Mr Newall had put forward would be very convenient in affording a guide as to the construction of stellar spectroscopes. It was a little disappointing to find there is so small a margin for further improvement, unless it can be obtained by improving the quality of the glass, and he imagined one of Mr Newall's objects was to stimulate others in the direction of such improvement. Lord Rayleigh presumed the only other way in which an improvement could be got was by taking our instruments to more favourable localities where the atmospheric disturbances were less, and the stability of the stellar image correspondingly greater.

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hold out very much hope in that direction; there seemed to be no immediate prospect of doing it, and apart from the results of experience, there was a theoretical reason for this conclusion also. For these prisms glass of considerable dispersive power was required, and unfortunately dispersion and absorption very largely go together. All accidental absorption due to the presence of colouring matter, or anything of that kind, could be eliminated, but he doubted whether it was possible to go very much further in that direction. It might be possible to use a prism or a train of prisms of one kind of glass for one end of the spectrum, and of different kinds of glass at the other end of the spectrum, since the absorptive power of various glass varied very much with the end of the spectrum at which they were used. It was possible to produce glasses which had very considerably greater transmissive power for the red end of the spectrum and others again which had a very much greater transmissive power for the violet end, and this might be usefully considered in connection with spectroscopes.

[The Editor having been good enough to let me see the proof-sheets of the discussion, I take this opportunity, which time did not allow at the meeting, to say, in reply to Dr Clay's question, that in the case I deal with in my treatment of the subject—namely, tremor discs of stars—the effect must be proportional to the quantity of light collected; that is, to the square of the aperture. Unfortunately, the ideal case referred to by Dr Clay very seldom occurs in visual observations, and still more seldom in photographic work. Even when the distribution of light in the diffraction image of a star approaches that indicated by theory, the atmospheric conditions bring about a ceaseless movement of the whole diffraction image over an area many hundred times as great.—H. F. N.]

POLISHING OF GLASS SURFACES.

By Lord Rayleigh, O.M.[1]

What I have to bring before the meeting to-day is not absolutely new, for I have already used some of the material in a lecture at the Royal Institution (*Proc. Roy. Inst.*, xvi. p. 563, 1901; *Scientific Papers*, iv. p. 542); but I do not think the remarks I then made attracted very wide attention, and I was wishful to bring the matter before a meeting like this, where there are present so many not merely theoretically but practically conversant with optics, and the more so, as some conclusions which I have put forward appear to stand in need of confirmation, or perhaps of correction. From a theoretical point of view there is no great difficulty in treating the question of polish. We may consider the standard surface to be a corrugated one—corrugated in a regular manner, and we may enquire how the reflection of light—or sound for that matter—is affected by the corrugations, how far the reflections differ from what they would be supposing the surface were absolutely plane. Here the question assumes a specially simple form if we limit ourselves to the case in which the medium is impenetrable to the vibration, in other words, acts as a perfect reflector. In the case of light we may think of silver as representing a perfect reflector. In the case of sound almost any solid or liquid body with a continuous surface plays a similar part. Consider, then, a corrugated surface bounding a material having complete reflecting power, and then we shall find that the question turns entirely upon the relationship between the period, as I will call it, of the corrugation—the distance from ridge to ridge along the surface—and the wavelength of the vibration that is being reflected.

This question was considered long ago by Fraunhofer in connection with gratings; and I may remark parenthetically that it seems to me he has never really had full credit for his work in this direction. It is well known that if the lines of a grating are closer together than a wave-length of light the ruling has no effect upon light incident perpendicularly. The spectra that would be formed with a grating less closely ruled are, as it were, pushed out of the field and nothing of them is left. It was upon this that Fraunhofer founded a conclusion as to the limits of microscopic power, and to my mind his argument was perfectly sound, as well as his conclusions, if we make a slight correction in it. Fraunhofer, I think, did not quite correctly treat the case of oblique incidence. We know from the ordinary theory of gratings that if the light is oblique the last spectrum does not disappear until the

distance between the lines, the period of corrugation, is as small as half a wave-length ; so that there has to be a greater degree of closeness if obliquity is admitted.   Taking the case in which the medium itself is perfectly reflecting, if there are no diffraction spectra formed it follows at once that the whole of the light must be concentrated in the beam specularly reflected, and thus the corrugation has no effect whatever.   The surface, however deeply corrugated, is perfectly polished ; no light can depart from it in any false direction.

From that we may obtain a very good general idea of what is necessary in order that a surface generally should appear polished, although the simplicity of the two dimensional cases will be somewhat departed from.   It is all a question of the relation to the wave-length.   If we are dealing with sound, where wave-lengths are comparatively long, then a very rough surface indeed is sufficiently polished to act as a perfect reflector.   In fact, we might artificially roughen a surface with attached pebbles, and yet leave it smooth enough to act as a perfect reflector for sounds belonging to the middle region of the musical scale, and the reflection would be as complete as if the surface were mathematically smooth. The pebbles would not act as a defect of polish.   But when we come to light, of course, the case is very different.   But even here it is a question of what kind of light we are speaking of.   A surface may be fairly well polished for red light and fail most decidedly if we use it for the reflection of blue light, or for the more especially photographic rays beyond the blue.   A surface that would be very imperfectly polished for those rays which affect the eye may be practically well polished for the dark heat rays which are found in the spectrum below the red.   In fact, I once made an experiment in which I used a ground glass not polished at all, silvered over the roughnesses, and reflected with it the light from a Welsbach mantle used without a chimney.   I found that in that way I could get a very good approach to complete specular reflection for dark radiation from a surface that would not count as a polished surface at all.   The test was made with a thermopile.

I need not explain to such a meeting as this what are the practical processes of grinding and polishing glass, but I wish to raise a question as to the difference there may be between the two operations.   Herschel, than whom in his day there was no greater authority, held the view that the polishing operation and the grinding operation were of one and the same character ; that in the grinding—I am speaking of working a hard surface—lumps of glass were broken out by the emery with which the glass was brought into close contact under pressure, and that in the polishing a similar operation was still going on, although on a much smaller scale.   Herschel expressed surprise that it is possible at all by means of art to reach a surface that shall be polished according to the necessary standard, that standard being set by the wave-length of visible light.   My own observations upon grinding and polishing glass led me to rather a different view.   It appeared to me that the operation of polishing as conducted with rouge embedded in pitch or carried upon a softer material like cloth or paper, was essentially of a different kind from the operation of grinding.   I followed the process under the microscope, which is easily done, especially if the surface is smeared over with a little aniline dye. It appeared that under the polishing no visible pieces of glass are broken away at all.   The polishing begins naturally upon the eminences left by the grinding, and in a very few minutes it produces little facets on the top of those eminences, and these soon reach a size sufficient to allow of a certain degree of regular reflection.   After five minutes polishing of a ground surface I found it quite possible to observe the interference rings between the very slightly polished glass and a flat and fully polished one brought into juxta-position with it.   As the polishing proceeds, the area of the facets increases and new ones are developed ; but the point I wish to emphasise is that there is no progress in the polishing.   The polish is perfect, where it exists, from the very first.   So long as the area of the facet was visible at all under the microscope, I was never able to see any structure in it.   It appears that the progress of the polishing consists only in extending the area of the polished surface, but not at all in improving the polish in any part that has been once polished.   In that case we must take a different view from Herschel as to the character of the process.   We can no longer suppose that pieces are broken out under the polishing analogously to what happens in the grinding.   It seems to me rather that the process is a molecular one, or nearly a molecular one, the upper layer of molecules probably being operated upon by the polishing

material. That structure cannot be seen under the microscope is no proof that there is in fact no structure until we come to the molecular limit; but the impression produced upon me was that the two processes were so discontinuous that it was a very natural conclusion that in the polishing process the material was acted upon molecularly. That is one of the points which I wish particularly to raise to-day, because I am not a practised microscopist, and the observation is one that can be easily made by anybody who is accustomed to work glass, and the question seems to me an important one that should be definitely settled.

In what I have said it will be understood that I have been speaking only of hard materials. Mr Beilby has made very interesting observations upon the polishing of softer materials, such as metals, and he holds that in that case the polishing process consists not merely in removing the eminences, the parts which are too protuberant, but in filling up the pits with the material removed from the eminences. In that way it is easy to understand that a very much more rapid approach to uniformity would be obtained; but I must say that in my experience with glass —and my experience was limited to glass—I never saw anything to suggest that idea, and my impression is that no material once moved is deposited again, and that the process of polishing has to be continued until all the glass is worn down to the level of the deepest pits.

I have a slide here, made under the microscope, of a piece of glass examined towards the latter part of the process of polishing. The pits are shown dark and between the pits there is no structure to be seen. These parts are entirely polished. From the amateur's point of view the polishing may often be terminated at a much earlier stage than would commend itself to a professional optician. I have made lenses which worked very well for my purpose, although certainly one would complain if anything of the sort were provided to one professionally, but I have never been able to see that their performance was any worse. The parts not polished are a very small part of the whole, and if it is a question only of the amount of light, I do not suppose the quantity dispersed by the spots would be missed. It might be another matter if it were a question of getting a very dark field or seeing a faint object. I am not suggesting that telescope glasses should not be properly polished, but pointing out that in many cases a very inferior polish suffices. Of course I only speak as an amateur. In that way not only is the process very much quicker, but there is also less danger of losing the figure of the surface, which is, I think, usually accurate enough at the termination of the grinding, but which is liable to be lost in the process of polishing.

As to the amount of material that has to be removed in the polishing of glass I made some observations. Some were made by weighing the glass to find out how much material it had lost during the operation. I started with a very finely ground surface, rather more finely ground I think than is usual in practice, and I found that in order to obtain a pretty good polish it was necessary to remove a weight of glass, corresponding to a depth of about 6 wave-lengths. I do not pretend that such a polish would satisfy the requirements of commerce; probably the 6 would have to be raised to 10 or 12 in order to get down to the bottom of the deepest pits. In another case I used a disc of glass very finely ground and polished in the lathe in rings, the object being to obtain a gradually improving polish over a finite width, increasing from the margin, where very little polishing was done, to the centre of the annulus, where the polishing would be at its best. When these rings were examined under the microscope it was found there were very few pits left in the middle of the ring, and I was able to obtain the result, easily verified by forming interference rings between this and another flat glass, by removal of material to a depth of only 2 or 3 wave-lengths.

I have one or two slides in which bands are shown obtained in this way. This first was from a glass sold to me as flat, and it was combined with another which was at any rate much better than itself. The rings were formed with sodium light and the photograph was taken upon a plate sensitised with cyanine. Although you see the figure of the glass was not bad from the point of view of general sphericity, the spherical surface was very far indeed removed from the plane.

This next slide is from a piece of ordinary plate glass in which the surface is saddle-shaped, showing therefore hyperbolic bands. Observations of this sort are very easy, and it is not difficult to obtain the

photographs. I may mention that I made in connection with this subject some curious observations upon the effect of treating glass surfaces with hydrofluoric acid. The acid was very dilute, the commercial acid diluted perhaps 200 times and kept in rapid motion in a bath. I found that so used the acid acted much more regularly than I had expected, or I believe would be generally expected, and it was perfectly possible to eat away the surface in a regular manner to any required small depth such as half a wave-length. I was able in that way to prepare rather pretty patterns by etching two flat surfaces in stripes and afterwards combining them crosswise, the depths being so chosen as to give the most brilliant colours of Newton's rings. I have a slide which shows the effect of contact with a drop of very weak hydrofluoric acid. This plate had been subject to various experiments, such as longitudinal polishing, but you will see the place where the drop of acid has stood for a few minutes, having the appearance of a hump. It is really a depression in the original glass, and by counting the bands one can see what the amount of depression was. It is about two bands, which would mean one complete wave of sodium light. In those cases we start with glass originally polished. Some very curious observations were also made upon the effect of hydrofluoric acid upon glass originally finely ground. The acid acts in such a way as to eliminate from the roughened surface all the finer irregularities, leaving only those of longer periodicity. It is not difficult to form a theory and to illustrate that theory by drawing especially if one takes the case of two dimensions. If one assumes, as seems reasonable, that the hydrofluoric acid always eats in normally to the surface, then if we start with any particular surface, and imagine that from every point of that surface spheres are drawn having their centres in the surface and a radius proportional to the time during which the hydrofluoric acid acts, then the envelope of all those surfaces will be the surface to be expected at the close of the operation. You see the ultimate result will be to leave the surface in the form of spherical segments, the centres of the spheres corresponding to the deepest places in the original roughly ground surface. I have a slide showing the appearance of such a piece of ground glass after treatment. It appears to be divided into a number of cells, and the wall of each spherical segment separated from its neighbour is of the nature of a ridge. Each cell is itself absolutely devoid of structure ; it appears finely polished. Although the surface was originally finely ground all over, all the minor irregularities are gone, and we are left with the surface which in a sense might be called polished, although, of course, it is far from flat. In each case, if we are looking down upon the surface, the middle of these segments will be the deepest place ; the ridges are raised and exceedingly sharp. According to the theory I have briefly sketched they would be mathematically sharp, and such they appear to be.

The character of the surface so obtained is illustrated by another photograph made with a slightly different focus of the microscope. The light comes from a paraffin lamp across which two wires were stretched. Under the focus adopted, each of these spherical segments acts as a concave lens. The focus is not the same for all, but they give material for calculating what the concavity was in each case. I found I could make a very good estimate of the exact shape and size of the various cavities.

Mr ROSENHAIN said that since reading the earlier accounts of Lord Rayleigh's views on polishing, he had been engaged upon experiments on the same question, having found further stimulus in the work of Beilby and Osmond. The observations he (Mr Rosenhain) had made gave results entirely supporting the views of Lord Rayleigh, with perhaps one exception, namely the question of the existence of surface flow in glass under the action of polishing. He (Mr Rosenhain) believed that there was a certain amount of filling up of holes in the process of polishing, not by the removal of particles of glass from one point and their re-attachment at another, but by a species of surface smear in the direction of polishing. This belief was based upon an experiment made on a piece of glass which had been purposely polished in a manner that would be unsatisfactory in practice, viz., by rubbing in one direction only. When such a piece of polished glass was attacked by hydrofluoric acid, with precautions similar to those used by Lord Rayleigh to ensure uniformity of attack, microscopic symptoms of striation could be detected which had not been visible before the glass was etched. This evidence was similar to that used by Beilby, Osmond and others, to prove the occurrence of surface flow in metals. If on the strength of

such evidence the analogy between glass and metals might be carried still further, it would be reasonable to suppose that the layer of glass altered by the process of polishing extended to a considerably greater depth than Lord Rayleigh seemed to suppose. In the case of metals this depth was very appreciable, Mr Rosenhain's own observations leading him to regard it as considerably deeper than Mr Beilby had supposed, Mr Rosenhain having recently shown that the surface smear was thick enough to be quite opaque. This had been proved by observations on a polished surface consisting of two metals, copper and iron, differing in hardness and colour. Some of the iron had been smeared over the copper, and even after etching with picric acid, the smeared portions of iron superposed on the copper showed no difference of colour from the solid iron, thus proving that no light had been able to penetrate the smeared metal and return to the surface. Mr Rosenhain considered that the difference in hardness between iron and glass was not really very great; between hard steel and glass he was not certain if the difference did not lie in the other direction, particularly in the case of some of the softer glasses, and he consequently believed that the action of polishing produced a distinct change of structure to a finite depth. Further evidence for this view could be found in the fact that the polarising effect of artificially polished and of " fire polished " glass surfaces was not quite the same, fire polished surfaces being those in which the surface layer had attained its approximately plane configuration during solidification from fusion, under the action of surface-tension forces. But the difference between such fire-polished surfaces and those produced by mechanical means was only a question of degree, since, on the theory of Beilby, Osmond and others, the re-arrangement of the molecules in the surface layer during the process of polishing is very similar to what would take place in a fluid layer arranging itself under the influence of surface-tensions. The foregoing remarks embodied what he (Mr Rosenhain) regarded as a very slight modification of Lord Rayleigh's views, and he felt that even as far as he had gone there was great need of further enquiry on lines that seemed fairly evident.

Mr Rosenhain then considered the question of the part in the polishing process played by the polishing medium itself; he believed there was an action which, for the sake of brevity and without committing himself to anything, he would call a chemical action. He (Mr Rosenhain) considered that there was some connection between the nature of the surface produced and the chemical nature of the polishing agent which had been employed, so that it might even be possible to modify the keeping qualities, the chemical stability of the surface by adopting a suitable polishing medium. He (Mr Rosenhain) did not know whether it would ever be possible to show analytically the presence of a chemically different layer upon a polished surface, but there is a fact well known in practice which bears upon this question. By allowing the conditions of moisture, pressure, temperature, etc., to become abnormal, the surface of the glass, instead of being further polished, is actually roughened, and the smooth surface, instead of being produced on the glass is produced on a layer of the polishing medium. This phenomenon merely emphasises the fact that there is some chemical or atomic action between the polishing medium and the substance being polished, and this gives rise to the hope that the properties of the surface may be modified by a change of polishing medium. Mr Rosenhain pointed out that there were a number of polishing media besides rouge and the other substances ordinarily used by opticians. Le Chatelier had shown the use of precipitated alumina, and the earth oxides also act as efficient polishing agents. The mere appearance and sensible texture of a powder was, however, no guide to its value as a polish. With regard to the etching action of hydrofluoric acid which Lord Rayleigh had illustrated by a beautiful lantern slide, Mr Rosenhain drew attention to a well-known operation in the glass industry which Lord Rayleigh's experiment would explain, viz., the " polishing " of cut flint glass. The glass is ground very finely on the wheel, and the polish is obtained by simply immersing the article in an etching fluid consisting of hydrofluoric acid and some saline constituents; the actual composition of the baths being, no doubt, very much a matter of rule-of-thumb. The articles go in mat and come out bright; no doubt microscopic examination will reveal the minute spherical surfaces described by Lord Rayleigh.

Mr HORACE BECK had been much interested in Lord Rayleigh's paper, as he had been particularly interested in the subject of polishing in his own works. He (Mr Beck) certainly thought that the

burnishing action to which Mr Rosenhain had referred did occur at times, especially when a somewhat rough surface was very vigorously polished. Under those conditions a surface is produced which appears to be practically continuous, but beneath which pits remain visible. Mr Beck could only account for this by supposing that the glass was smeared somewhat in the way suggested by Mr Rosenhain. With regard to the amount of polishing required before a surface would show regular reflection, he (Mr Beck) had found that when a lens had been extremely finely smoothed, it was sufficient to wipe the surface with a cloth to obtain the colours when tested with a proof glass. In this connection it was well known that under the old method of polishing, when proof glasses were not used, many workmen, who declared they were able to do the best work, and able to get a fairly good figure when the glass was only half polished, were quite unable to maintain good figure when really complete polish was required. The device of polishing only slightly is used for testing lenses that have been made on calculations which are not absolutely trustworthy; just enough polish is put upon the surfaces to be able to see through them, and they are then tested; it is astonishing how much light does come through under such circumstances.

Mr Beck had recently gone into the question of unequal polishing, with a view to producing glass surfaces that would act like the well-known Japanese mirrors; he wished to obtain a glass reflector that would show a pattern, and he found that by covering a portion of the partly-polished surface with a very thin protecting coat, and then continuing the polishing, after a time removing the protecting coat and then completing the polishing, such an effect could be produced. The surface of the glass when looked at showed no signs of the pattern, but the reflection showed it plainly.

Mr Beck considered that there was no doubt that a different class of polish could be obtained by using different polishing materials; workmen always consider that a much more burnished surface results from the use of tripoli than when such materials as rouge and putty-powder are used, and he thought that a surface slowly polished was much more perfect than one rapidly polished.

The phenomenon of finding the polishing material working into the glass and polishing over the material was continually occurring in practice, although he (Mr Beck) could not recall the workshop name for it. Acetic acid is used to remove the defect, and he (Mr Beck) considered that in view of the fact that acetic acid will so largely remove the coating, the amount of glass affected could only be very small.

Mr F. Twyman wished to raise a point which he had already referred to, but which received additional interest from Lord Rayleigh's paper; he referred to the strain which is set up when a polished surface is made grey. If a very thin piece of glass, grey on both sides, be polished so that it can be examined through its thickness, and polarised light be applied, strain can be distinctly seen near each surface; if now one side be polished, the strain disappears from that surface and the glass blows up; on polishing the remaining grey side the glass becomes parallel again and the strain entirely disappears.

The Chairman (Lord Rosse) in proposing a vote of thanks to Lord Rayleigh remarked that he had often found it very difficult to obtain a polished surface, although he would be satisfied with surfaces which would not be looked at if they came out of an optician's shop. His (Lord Rosse's) difficulty was to avoid scratches; in a workshop where a separate room is devoted to each process and where one workman does one thing all the time, and does not go about among sand and emery, there is a much better chance of obtaining a good surface. His (Lord Rosse's) experience coincided with that of Lord Rayleigh to the effect that a surface which does not look good to the eye is superior to one with a very perfect polish but not correct otherwise. In polishing flats he (Lord Rosse) had found some with a good many scratches, but which, when tested at night with a telescope, gave very good definition. One of the objections to imperfect polish seemed to be not so much the loss of light as the diffusion of light; in looking for a small white object like a star, the more diffused light there is in the field, the more difficult it is to see the object; this diffused light might be due to the atmosphere or partly to the defects of the surface. In working with metallic surfaces we were always under the disadvantage that a very good surface after a very short time begins to tarnish and to show a good deal of diffused light. Working with a mirror night after night in various kinds of weather, a deposit of dew inevitably occurs, and then the surface is never the same until it is re-polished.

THE PARALLEL PLATE MICROMETER.

By J. H. POYNTING, F.R.S., Mason Professor of Physics in the University of Birmingham.

IF a parallel plate of glass is interposed between the objective of a microscope and the object, the image is seen in its true direction when the plate is perpendicular to the axis. When the plate is tilted the image is shifted sideways, and by an amount which, for angles less than 10 degrees, is very nearly proportional to the tangent of the angle of tilt, and for such angles the definition is not appreciably impaired by the tilt when a low power is used.

To use the plate as a micrometer it may be fixed to one end of an axis which turns in bearings, and is perpendicular to the axis of the microscope. A pointer attached to the revolving axis moves over a straight scale, and the number of divisions of the scale from the centre is proportional to the tangent of the angle of tilt, and therefore to the shift of the image. Suppose that it is required to measure the diameter of a small particle *ab*. The plate AB is tilted so that one side of the particle, *a*, is on the crosswire in the eyepiece, and the position of the pointer on the scale is read. The plate is then tilted so that the other side of the particle, *b*, is on the crosswire, and the position of the pointer is again read. The difference of the two readings gives the diameter in scale divisions. The value of a scale division may be determined by using as object a finely divided scale.

The micrometer may be entirely detached from the microscope so that in manipulation there is no risk of disturbing the microscope. There is no backlash.

For powers higher than $1\frac{1}{2}$ or 1 in. there is insufficient space for the plate between the object and the objective, and the tilting affects the definition. It may then be interposed in the tube between the objective and the eyepiece, and in this position it is, of course, much more sensitive, while the definition is practically unaffected.

In a measuring bench or comparator in the Physical Laboratory of the Birmingham University, we use two microscopes with 2-in. objectives. The plates are 6 mm. thick, the pointers are 25 cm. long, and move on millimetre scales with about 100 divisions for a shift of 1 millimetre.
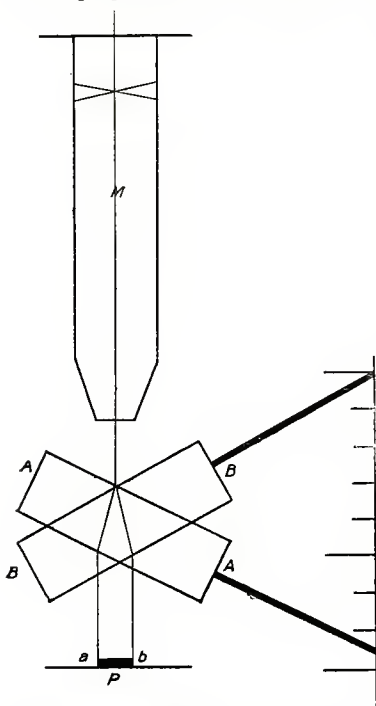
Fig. .

The parallel plate micrometer was described by Clausen as far back as 1841 (*Ast. Nach.*, xviii., 1841, col. 95-96). It was re-invented by Porro in 1842 (*C. R.*, xli., 1855, p. 1058). Porro used both the form described above and a double image form for the telescope. In the double image form there are two plates, each occupying half the field placed in front of the eyepiece. One is fixed and the other can be tilted about an axis perpendicular to the line of division of the plates.

I have used both forms, and I find both of them exceedingly convenient, rapid, and accurate. The parallel plate micrometer is easily constructed, and is inexpensive. It merits more notice and much more use than it has yet received.

LORD RAYLEIGH.—Since Professor Poynting's re-invention of this instrument, I have used it constantly with a cathetometer, and have found it very useful.

THURSDAY, JUNE 1.

SECTION II.

Mr W. A. DIXEY in the Chair.

SOURCES OF ERROR IN OPHTHALMOMETRY, WITH SUGGESTIONS FOR THEIR AVOIDANCE.

By J. H. Sutcliffe.

Although the science of ophthalmometry is based upon one of the simplest and most elementary of optical laws, there is a singular dearth of intelligent and explanatory text-books, in English, on the subject. Professor Tscherning has treated the subject with special clearness; but the fact that his text-book, "Physiologic Optics," treats only of the older models, is apt to prevent his work receiving the attention that it fully deserves.

Ophthalmometry is based upon the well-known law that the radius of curvature of a convex mirror can be easily ascertained if we know

　　　　1. The size of the object;
　　　　2. Its distance from the mirror; and
　　　　3. The size of the image.

An ophthalmometer is an instrument containing an object of known size, termed the mire or mires, and a telescope with an objective (which must be at a certain distance from the eye in order to obtain a clear image). Thus we have the first condition perfectly fulfilled, the second fulfilled with a negligible error, and the third the construction of the instrument should determine for us. The cornea of the eye is the convex mirror.

Let us take a white illuminated ring, about a quarter of an inch in thickness and eight inches in diameter, and hold it a few inches in front of a cornea. Through the centre of the ring put an empty tube, through which we will look at the image of the ring on the cornea. The average ophthalmometric object is usually 15 cm. in diameter, and the average human eye 7·5 mm. radial curve. We shall find a diminished image of the object, but so small that we can scarcely examine it. We therefore put in the tube a system consisting of an objective and an eyepiece, and obtain a magnified image. We could, in the laboratory, arrange that the image formed by the objective coincides with a micrometer, and thus obtain the exact diameter of the image; but, on account of the tendency of the observed eye to move, and the stationary position of the micrometer, such measurements would be impracticable. All ophthalmometers are constructed to give a certain definite size of image, whatever be the curvature of the eye.

If we could rely upon the micrometer system of measurement, it would only be necessary to vary the size of the object and produce an invariable size of image, no matter what were the curve of the eye. The curvature of the eye could then be accurately estimated by measuring the size of the object

—a comparatively easy matter. This, however, is impossible in practice, on account of the mobility of the eye.

In order to obviate this difficulty, Helmholtz & Young introduced the ancient astronomical system of doubling, without which ophthalmometry is practically impossible.

In the telescope let us place two prisms, bases in, and vertical, and we shall now see two circles in the place of one. We could so arrange the position of the prisms that when used with a cornea and lens of known curvatures and an object of known size, the two images of the circle circumferences would come into contact. That position once fixed, we should find, on substituting an eye of lower curvature, that these images would now overlap; but if the eye were of higher curvature, the images would separate.

There is practically little difference between the various existent ophthalmometers, such improvements as have been made of late years consisting of mechanical adjustments and methods of illumination and patterns of mires.

The chief difficulty appears to have been an accurate system of doubling.

Doubling can be obtained by using :—

1. The inclined plates of Helmholtz.
2. Weak prisms with bases in, inclined at an angle. (Kagenaar.)
3. A pair of prisms in one plane with bases in.
4. Cutting a strip out of the objective to decentre it.
5. Slicing up the objective and displacing each half laterally.
6. Cementing a single small prism in the centre of a larger plano lens.
7. Cutting a hole in the centre of a prism.
8. Using the bi-refringent prism of Wollaston.

Each maker has his own system. The last is preferred by many physicists, on account of the images formed being two complete cones, whereas all the others are two half-cones. The expense, however, of the Wollaston prism is a deterrent.

The Variations of the Doubling.—As the doubling of the image is of little practical utility if we cannot vary the separation, all ophthalmometers are so constructed that with a definite adjustment, contact takes place between the images with an eye of standard curvature, and all other curvatures cause an overlapping or a separation of the images; nearly all ophthalmometers give the amount of variation from the standard eye-curvature by an estimation of the mechanical movement necessary to enlarge the separated images or diminish the overlapping ones, so that the images just come into contact. For example, in the Javal-Schiotz ophthalmometer, an adjustment of about 5 millimetres represents a difference of one dioptre in curvature.

In all ophthalmometers the idea is to alter the size of the object. This alteration or variation in doubling may be secured :—

1. By moving *one* of the mires laterally, thus altering the size of the object. (Javal-Schiotz.)
2. By moving *both* laterally. (Davies.)
3. By moving the telescope prisms longitudinally and so securing approximation and contact (stationary mires and movable prisms). (Chambers-Inskeep.)
4. By superimposing the image of one mire over the other (stationary mires and stationary prisms). (Hardy.)
5. Varying inclination of the doubling plates. (Dubois.)
6. Varying prisms. (Brudzewski.)
7. Alteration of the circumference of object with an iris diaphragm. (Reid.)
8. Also by another method, details of which I give below.

The Mires.—There is an infinite variety of mires. The two mires usually found in ophthalmometers must be considered as one object, *i.e.*, a ring of which every portion has been obliterated except two small portions on opposite sides of the centre, so that when we are looking at these two portions, it is

simply the diameter of the ring. This gives us the reason why duplication is necessary, because there is really only one image to be doubled, and although there are apparently two objects, and the duplication makes four images in all, two (the outside ones) are useless for measurement, and are neglected.

Reasons for Differences between Subjective and Ophthalmometric Astigmatism.

I have given the subject very serious study during the last five years. Previously I had been surprised at the immense number of ophthalmometers bought and ultimately discarded as being nothing but instruments for the laboratory or the show-room. Showy appearance of the ophthalmometer may undoubtedly have been one of the causes of its popularity; possibly fewer might have been purchased were it as modest in appearance as an ordinary ophthalmoscope. I had always had great belief in the ophthalmometer as being an instrument of the physicist rather than the physician, and have come to the conclusion that many of the differences between the subjective and the ophthalmometric determinations of astigmatism are not due to the so-called accommodative astigmatism, of which I doubt the existence, but rather—

(*a*) to the variation of the astigmatism in the different zones of the cornea.

(*b*) to the personal factor of the observer—errors due to the parallax.

(*c*) To the observation of an aerial image.

(*d*) Position of trial and spectacle lenses.

I am convinced that, if these four sources of error could be avoided—and I believe that to a large extent they can—we should have in the ophthalmometer the most valuable and reliable instrument in refraction. Given also the elaboration of the ophthalmophakometer—an instrument for measuring the anterior surface of the crystalline lens, and the position of its centre—we should have an instrument absolutely independent of subjective tests, or of the necessity for mydriatics.

Let us take some of the sources of error I have mentioned, and see how they can be avoided. Briefly, the features I have introduced to overcome some of the above difficulties are :—

1. An object capable of covering with its image the whole of the corneal curve to within $\frac{1}{2}$ mm. of its centre. A mire independent of the focussing lenses.

2. The variation in the doubling is secured by moving the mires to and from the observed eye, instead of laterally.

3. The quadruplication, instead of the duplication, of the image.

4. The abolition of the eyepiece, and substitution of a ground-glass focussing screen.

Sulzer made a long series of experiments, showing the cornea to have an even curvature in an area of about 4 mm. diameter. After that the curvature becomes irregular. The average ophthalmometer gives an image of about 3 mm. in diameter, and neglects the more central parts. It is assumed that the central portion is exactly like that $1\frac{1}{2}$ mm. from its centre; but an instrument capable of measuring, say, 1 mm. or less from the centre, and also of measuring 4 mm. from its centre, would be able to give an average that would make up for much of the subjective and corneal difference.

The instrument I have to show you has been entirely constructed by myself, is naturally amateur in pattern, and does not by any means convey the idea of the ultimate mechanical construction. For demonstration purposes only I have made a wooden model, which will show you how the different ideas that I suggest can be carried out in a perfect instrument, and I think will do much to remove the above sources of error. You will notice that the mire can move longitudinally, and independently of the focussing arrangement. Thus I can obtain as large an image as I like on the cornea, and also a smaller image. It is as easy, as you will see on examination, to produce an image 6 mm. in diameter as it is to make one of 1 mm. in diameter, and it will remain in focus. If I focus the telescope so that the equal image is clearly seen, and move the mire longitudinally backwards and forwards, the image would cover the whole surface of the cornea, and by its curving or irregularity at different portions, show whether there is any irregularity from the very centre to

the extreme border. I am not aware that any other instrument does this. By having an adjustable focus in the telescope it is possible to measure the amount of astigmatism of any portion of the cornea. This, however, would only be useful in experiments in the laboratory; but in cases of irregularity it might be interesting to strike an average. It would most conclusively diagnose irregular astigmatism.

The variation in the different zones certainly does play an important part in the resultant astigmatism, and if this could be measured we should find it useful where there was doubt. I think that this suggestion in the instrument will completely avoid the errors described under this heading.

I have come to the conclusion, from an experiment I made with one of the most modern of ophthalmometers, that the personal factor of the observer is responsible for most of the differences. Examining a glass eye as a model for a demonstration, I came to the conclusion that the astigmatism was 0·75. Six men who were all accustomed to ophthalmometers sat down in the chair after me, and examined the same eye. Nearly all the results varied by about ·25, most of them also in the axis. Surprised at the results, I sat down again, and found that my results were different to my previous finding. On examination, however, I found that I could alter the result by 0·25, and even 0·50, by moving the head slightly to one side during the observation. I ascribed this fault to inability to focus sharply when using the ordinary long-distance lenses of the telescope, and also to the fact that, when using the eyepiece, one has to look directly through the base lines of the prisms; and if by chance the eye is directed to one side of the prisms, there is a tendency for one of the mires to alter at the expense of the other, thus leading to wrong conclusions.

I also put a certain amount of difference as being due to the strain forced upon the observer's eye when looking through the eyepiece of the instrument, a fact in which most microscopists will bear me out.

The cure that I suggest, which I have embodied in the rough model of this instrument, is first to do away with the aerial image by projecting the image on to a screen, which must be of an extremely diaphanous nature, and to use fairly high power lenses in order that the focussing may be delicate, so that if the eye is thrown out of focus, one knows immediately the distance from the cornea of the observed person to the mire is not correct, whereas with the longer distance lens there is a certain latitude and laxity in focussing which upsets this distance. By using a screen I find that the observer can move his head slightly to one side practically without affecting the result. Again he is not under any strain, and there is a peculiar effect noticeable, for which it is difficult to find a reason, that a very fine grained screen interposed at such a place very often gives more detail than would that of an aerial image. With a screen I have found that the personality of the observer is not so much in evidence as it is comparatively easy for a layman to focus an image on a screen and observe its detail.

Very often it happens that a careful observer will make perfectly correct measurements in finding what is known as the first position in astigmatism, but by being compelled to turn the mires round to the opposite axis, distracts the attention of the observed eye, and causes it to move its position, thus making the second determination in an irregular condition. It is quite possible for an error of at least one dioptre to creep in. To obviate this difficulty I have practically done away with the revolution of the mires by a novel principle in ophthalmometry, i.e., quadruplication. To enlarge the images I push the mire close to the eye, to reduce them I pull the mire further away without affecting the focussing arrangements, so that I can secure contact with any shaped or sized image.

The telescope has no eyepiece for reasons above given. Instead it has an ordinary projecting arrangement throwing the image on a specially prepared screen, which is in turn examined by a weak enlarging lens. Naturally one difficulty connected with the production of a final image six or seven times larger than the original corneal image is, under ordinary circumstances, the employment of a very long and clumsy tube. To obviate this difficulty I have had recourse to one of two different methods; either to the telephoto principle, or to the reflecting prism method, which latter arrangement I have here. For the quadruplication, in the telescope behind the lens is a series of prisms which multiplies the

original image four times instead of twice, thus giving on the screen four circles. With an astigmatic cornea this would give four ovals, which could be approximated or separated without any revolution of the mires after their first position. I must apologise for the extremely rough construction of the instrument, of which I hope a fully completed model will be put before you on no very distant date. There are naturally certain improvements which are more of a mechanical than scientific kind, but my idea in bringing forward this extremely rough model is rather that you should see the principle that I have brought forward in order to avoid common sources of error, and that you might witness for yourselves that it is possible to produce a minute image with sufficient intensity upon a screen, and with little trouble. I am much indebted for the loan of lenses &c., to Messrs Aitchison, Dallmeyer & Dixey, and to Professors Tscherning, Silvanus Thompson, and Mr Chalmers for optical assistance.

ACCOMMODATION IN CASES OF ASTIGMATISM.

By H. L. Taylor.

BEFORE entering into any details of this subject there are several points calling for remark. In all I have to say I shall assume that the astigmatism is of the "regular" variety, that the eye in every case has not worn the proper or any correcting lens, and that the mechanism of accommodation is in accordance with the Helmholtzian theory. Further, I shall not discriminate between corneal and lenticular astigmatism, but deal with what we may call the "resultant" amount, as ascertained by the usual methods of trial lenses or retinoscopy.

In the astigmatic eye there are two principal meridians, one of highest, and another of lowest refraction, these being normally at right angles to each other. Let us for simplicity also describe the most faulty meridian as being either vertical, horizontal, or oblique, allowing a slight deviation from either V or H to be reckoned as the type. You will observe that I have used the expression "most faulty meridian." I have done so because in myopic astigmatism it will be that of highest refraction, whereas in hyperopic it will be that of lowest, mixed astigmatism being reckoned with myopic for this purpose.

In simple myopic and hyperopic astigmatism it is possible to see distant lines clearly by static refraction, so long as they are parallel to the faulty meridian. In compound hyperopic astigmatism it is not possible to see distinctly lines in any position, or at any distance, without using the accommodation. In compound myopic astigmatism it is impossible to see any distant lines clearly, and the use of accommodation only blurs them more; but in mixed astigmatism lines in a certain direction may be distinctly seen by dynamic refraction, if the amplitude of accommodation will allow it, but those at right angles will then be blurred to a greater extent.

I do not think it is necessary for me to go further into such details, which you doubtless understand, but I may sum up the matter by saying that by the aid of accommodation it is possible, within well-defined limits, for an astigmatic eye to focus certain lines, provided they are within the far point of the most highly refracting meridian, if you will allow me to use the words "far point" in this connection.

The astigmatic eye cannot, then, by any ordinary effort of the ciliary muscle acting upon the crystalline lens, focus lines in any direction, so as to get equal definition, because any ordinary added sphericity is merely like changing a sphero-cylindrical lens for one of higher power, but of the same cylindrical value. Consequently, the relative distinctness or indistinctness of the various lines of the star chart remains much the same. And yet we occasionally find in evident astigmatic cases—but always, I believe, of rather low degree—that lines in all positions are apparently brought into focus, or at any rate can be made so definite as to give high visual acuity by subjective testing.

Various explanations have been given as to how this is effected, but the one which has attracted

most attention is that which I shall call "sectional accommodation," by means of which a cylindrical power is added to the crystalline lens instead of, or in addition to, the ordinary spherical accommodation, through the action of the ciliary.

Before discussing this part of the subject, which seems to be copied from text-book to text-book with little additional evidence or investigation, I propose to ask your attention to some facts which are, I trust, less elusive and more satisfactory because of their universality.

I have for some years been endeavouring to seek a solution of the difficulty along what I believe to be comparatively untrodden paths, reasoning, so far as possible, from the simplest known facts towards the unknown.

Let us turn for a few moments to the relation in position between objects in the outside world and the erect human beings, to the objects we look at, their form and position, and last, but not least, to the exigencies of our daily life, with all our inherited tendencies. For man himself, lines and outlines in a vertical position play a very important part, and we notice their predominance on every hand. Trees and vegetable life generally have distinctive features in this direction in obedience to natural laws, and mankind copies them in the lines of his dwellings and conveniences of life. Even the landscape appeals to him differently in foreground and background, for close at hand it is the tree trunk and the upright edges of cliffs and rocks which he notices, whereas only at a distance do the contour of hills, the undulations of the ground, and the spreading branches of trees become specially noticeable. His savage ancestor was threatened by danger approaching in the vertical position—the raised club or axe, or the animal charging—and when we find him sufficiently advanced to express thoughts by signs, we get picture-writing, with its many vertical strokes, gradually emerging into a written alphabet. Let us take our own and analyse it. I have here an illustration which shows the letters in the word "astigmatism" separated into their V and H components. You will see how readily the word can be deciphered from the one set, and how difficult—in fact, almost impossible—it would be to read it from the other. The first and second lines show the word with the two sets blurred in turn. You will observe these are "capital" letters; of manuscript and small print I shall have more to say later.

The perception of relief depends largely upon vertical lines, and this is the highest development of binocular vision—in fact, we may have binocular vision well developed and yet have this, its highest manifestation, deficient. It is here worthy of note that the eyes are better provided for lateral movements than for vertical ones—the field of vision is greater in that direction—and they also have a special conjugate movement of convergence to that end. Moreover, we have a far greater degree of freedom in movement of the head from side to side, and the same thing is seen in the movements of the body as a whole, for we cannot, even by taking thought, add the proverbial cubit to our height in the hope of getting a vertical movement. It is scarcely necessary to consider so much, for are not the two eyes placed horizontally? Zeiss has artificially enlarged this interpupillary distance in his stereo-telescope for the purpose of giving the effect of relief. If two cords stretched parallel with the ground, from four to five feet from it, and one in front of the other, be viewed, we cannot tell which is in front, but if the head be bent at right angles, so that the eyes are in a vertical line, there is a decided difference.

Then there is the vexed question of the horopter. There is fair evidence that the ground we stand on is a horopteric surface, and that lines running from the eyes to distant points are important. These, we notice, will be projected as vertical lines on the retina.

Finally, Stevens, by careful experiments with his clinoscope, has shown that horizontal lines are not held in fusion nearly so easily as vertical ones, the amplitude for each eye being about $3°$ each way, whereas the amplitude for the vertical is about $11°$ each way for each eye.

These illustrations by no means exhaust the instances which might be adduced to show the great importance of vertical lines in vision, and above all we must reckon with the inherited tendencies for many generations. This is so marked in some respects as to lead many authorities to believe in a number of inferior cerebral centres controlling some actions, as in the case of our infallible innervation

when we have to discriminate between crossed or homonymous diplopia, a matter in which our higher intellectual powers cannot give us information. Le Conte calls it "unconscious cerebration."

If, therefore, the inherited tendencies have caused the V lines to assume such prominence in the acquired factors of binocular vision, surely we may reasonably expect that the same inherited practice will be revealed in some way in the visual acuity of the eye. And our search is not unavailing, for the ametrope before the test type affords us proof. I think many of you must have been struck by the fact that the simple astigmatic myope who requires a cylinder with axis horizontal will oftentimes, even when his uncorrected defect is ·75 D or 1 D, read $\frac{6}{9}$ by Snellen's type. How different is it when the defect is against the rule, and a cylinder with vertical axis is required! Then we expect, with the same degree of defect, something like $\frac{6}{18}$ or $\frac{6}{12}$.

I will now ask your attention to the small prints of books and manuscript. You will see that these differ from the large type considerably. Mankind found the original alphabet too cumbersome for writing, and devised letters which had many more curves in them, and consequently a much larger proportion of the horizontal components. It is very instructive to notice that amongst European nations which suffer most from myopia we find their writing, and especially the usually long upright letters, approaching a position which favours the horizontal.

There is another feature of reading at ordinary distances which is worthy of note. With a certain amount of education we do not take cognizance of individual letters, but rather read the words as a series of forms, being guided by length and shape, only giving attention to components when we do not readily recognise the form. The predominance of the V lines will not be so evident.

These are the elements upon which we have to work, the salient characteristics of things seen by the eye, and it is well in considering any visual problem to bear in mind a very important principle—the principle of the least expenditure of energy in the attainment of a given result.

It is somewhat remarkable that optically the normal eye is far from perfect, a fact which gave rise to Helmholtz's well-known dictum; it is equally remarkable that mechanically its perfection is so great as to become bewildering to the student of the intricacies of its motor mechanism, to say nothing of the puzzling nervous supply to the iris.

Take normal ocular muscular action where you will, it all seems to be devoted to the attainment of certain ends by the expenditure of least energy. Accommodation is no exception to the rule. In reading we are continually exercising it over a certain region (*i.e.* region of accommodation) within the range of accommodation for which the eyes are adapted, because the central portion of a line of type will be differently placed from the beginning and end, and each of these in turn must be a different distance from either eye. I believe it is agreed that the eye furthest from the type being read decides the amount of accommodation used, provided the visual acuity of the two is equal in value.

I am inclined to think that were careful records kept of these cases which we sometimes meet with, in which one eye is used for distance and the other for reading, some difference in the position of the faulty meridians, or the nature of the astigmatism of the two eyes, was primarily responsible for the anomaly, the reading eye being more myopic and using less accommodation. You will not fail to see that the simple astigmatic hyperope, even of low degree, choosing to get better vision by clearness of the vertical lines, accommodates for them and blurs the horizontal, violating the principle because of the necessity of distinct vertical lines. It is further instructive to note that when the same astigmatism amounts to say 3 D, in some people not far from the presbyopic age, they still read fairly well because of the lessened importance of these, and the increased value of the horizontal. It is easy to make oneself artificially myopic in one meridian and superably hyperopic in another—say by a mixed sphero-cylinder—2 D Sph. + 3·25 D Cyl., and use it with the cylinder axis first H and afterwards V, viewing the star chart at a distance. This will mean that if we require to see vertical or horizontal lines, as the case may be, we must accommodate to overcome the artificial hyperopia. It seems fairly general that there is a much readier response for the vertical lines than for the horizontal.

Many authorities to-day insist that the horizontal lines are the most important in vision. The

statement seems to be taken from one to another, and I cannot find sufficient evidence to warrant the belief. If the conclusions set forth above be correct I believe that where accommodation is exercised in astigmatic cases, in the majority of instances the vertical lines claim attention, and an effort to obtain good definition.

Does sectional accommodation ever take place? It has been described under various names. Javal calls it "astigmatic accommodation," Clarke "meridional asymmetrical accommodation," and Tschyrning (an unbeliever), "dynamic astigmatism of the crystalline." The idea of such an action being possible seems to have originated with Giraud-Teulon, the coiner of that felicitous sentence "Sight is touch at a distance;" but it was Dobrowolsky who first sought to prove it about 1868. There can be no doubt but that the hypothesis came on the scene much before the entry of many facts, and one is reminded in so many of these cases of the saying : "The recurrent tragedy of science is the killing of a beautiful theory by an ugly fact."

Most of the familiar names in ophthalmometry are ranged for or against the theory. George Martin, Mauthner, and Javal believe in it, while Bull, Sulzer, Hess, and Tscherning reject it. Hess and Javal have, so far as I know, given us the most important contributions to the subject, both their own and others' work—Hess particularly ; and amongst works in our own language Clarke and Suter give brief arguments for and against respectively. Dobrowolsky experimented on himself; many accepted these experiments without further proof, nor can I find that Javal really investigated the matter at first hand, apparently being carried away by his researches in ophthalmometry, and finding in the theory an easy explanation of certain facts. George Martin, in his "Clinical Studies of Ophthalmometry," deals with the subject very elaborately, and distinguishes two varieties, which he terms "renitente" and "elastique," the first a permanent physiological state, which can only be shown to exist under atropine, and the second a less permanent condition which can be neutralised or suppressed by the use of cylindrical lenses, returning when they are discontinued. He believes that such astigmatic contractions favour the development and even the inception of myopia, and makes them potent factors in the production of the myopic crescent of the optic disc.

Outside the observations of clinical cases and deductions drawn from them, the use of atropine to demonstrate Martin's "renitente" variety and some recent experiments with purely local applications of homatropine and eserine, the main support of the theory is derived from Hensen and Voelker's experiments with needles inserted through the outer coat of the eye, these being performed on animals, and the cut ends of filaments of the ciliary nerve stimulated.

Hess seems to be the great antagonist. He experimented with two cotton threads stretched at varying distances, but always at right angles to each other, and examined a number of cases to determine the possibility of such a state as sectional accommodation existing. His conclusions are decidedly adverse, and he further investigated the sources of error into which his opponents might readily fall. These he tabulates as follows :—

    I. The objects employed in experimenting are generally too large.

    II. The eyes wander and accommodate for different lines in turn.

    III. With radiate figures the eyes may accommodate for a region in the astigmatic rays midway between the proximal and distal focal lines and so get equal definition.

    IV. By narrowing of the palpebral fissure diffusion circles are often greatly reduced.

Tscherning, in an article upon the theory of ophthalmometry of the cornea, disputed even the existence of Martin's "renitente" variety, and rather naively remarks that all these delinquencies are dumped on the crystalline because it is so difficult to get at, and cannot defend itself. He believes that this state is caused by the alteration in spherical aberration brought about by atropine, but one is tempted to think that Tscherning's objection is somewhat prejudiced by his attitude upon the whole question of accommodation, valuable as are his contributions to physiological optics.

There is a cogency and sequence about the objections raised by Hess which have given them great weight. The objection that the eyes may obtain definition by wandering and focussing the various

lines in turn seems to me to be an error, and one which is constantly being repeated in some form or other. I do not believe that the astigmatic eye can, by purely spherical accommodation, focus any lines distinctly except those indicated by the axis of its corrective cylinder or the meridian of its defect. This will be clearly seen by using a sphero-cylindrical lens before the eye to produce artificial hyperopic astigmatism and adding plus spherical lenses to imitate spherical accommodation. We know that many cases when in front of the chart seem capable of focussing any line, but I believe it is done in accordance with the third objection, *i.e.* the eye accommodates for a region between the distal and proximal focal lines, and so gets equal definition, or rather equal blurring. Moreover, most of these cases are of low degree of error, and in addition, as he urges in his fourth objection, narrowing of the palpebral fissure lessens diffusion circles.

That the use of atropine often reveals what seems to be a tonic sectional contraction of the ciliary is well known, and many cases are recorded where a lenticular astigmatism masking an opposite corneal astigmatism has disappeared with age. It is scarcely scientific to regard this as a proof of dynamic astigmatic accommodation, because there are other possible causes.

Javal expressly states that cases which he has met have rarely exceeded a half dioptre in the value of this function. He starts the argument with an admission that his view of the matter is hypothesis pure and simple. The ciliary habitually contracts so as to produce spherical accommodation, but it can possibly act otherwise, with a certain association between its component parts, just as with the hand it is impossible to close one finger upon the palm without bending the others more or less, and he maintains that astigmatic accommodation will always be accompanied by spherical accommodation.

To my mind, one of the greatest objections to the whole theory is a physiological one. If we suppose that only certain principal meridians are capable of this compensating adjustment we must, in turn, suppose nerve centres which adequately control certain sections of the ciliary. There seems to be no basis for this assumption, and as Suter remarks it is highly improbable.

We cannot deny, however, that a state simulating sectional accommodation exists in some cases. I venture to think that frequently it will be caused by muscular imbalance, or at any rate, by some anomaly of the extra ocular musculature. We really know very little about the exact lines of action of the forces exercised about the living eye. When we remember the intricacies of the relations of the recti and obliques with Tenon's capsule it is necessary to pause before denying that they may influence the shape of the globe. With these muscles there is an arc of contact, a part where the tendon and body touch the globe, and the pull exercised by any muscle when in action is exerted upon the point where it ceases contact ; but there must be a pressure upon the globe throughout the arc of contact. Consequently the slightest variation in shape would cause a variation in tension of the choroid in certain parts, and this being communicated to the suspensory ligament would naturally cause the lens to assume an asymmetrical shape. Presuming that through the action of the lateral recti the lens is released from pressure to a greater extent in that direction, we should get a compensating adjustment in many cases of hyperopic astigmatism.

In addition to the narrowing of the palpebral fissure lessening spherical aberration, as mentioned by Hess, I am persuaded that the lids may cause actual pressure on the cornea. The stare astigmatism mentioned, I believe, by Thorington, can readily be demonstrated by the ophthalmometer, and from some experiments in my own case I find that a steady and intentional stare at the star chart will affect the visual acuity and the meridian of astigmatism, and this is just the corrective action for hyperopic astigmatism with the rule.

To sum up the whole matter, I believe that cases of astigmatism obtain the best vision possible in one or more of three ways :

Firstly. By endeavouring to focus vertical lines, and, failing these, the horizontal, by the aid of spherical accommodation wherever possible.

Secondly. By obtaining by the same means blurred vision for all lines.

Thirdly. By extra ocular mechanical means, some purely physiological, and others learned by trial and experience.

The Chairman (Mr Dixey) considered that the question of sectional accommodation was of the utmost importance. He thought the *onus probandi* lay with those who had advanced this theory, and, in his opinion, it was not proven. He was not sure that Mr Taylor was justified in drawing the large inferences he did from the prominence of vertical lines ; it was, Mr Dixey thought, a subject for further consideration and debate.

BASE LINE APPARATUS.

By J. Agar Baugh.

[Abstract.]

Base lines are the foundation of all geodetic measurements, and great accuracy is of the utmost importance. The older methods, such as end rods of glass, steel, etc., or line measures of bronze, steel, etc., involve very great expense. More recently base lines have been measured by bi-metallic 10 feet or 3 metre bars, the temperature effect being compensated by the varying lengths of the two metals ; but the complete apparatus is very complicated, and a number of skilled observers are required. A great simplification was the introduction of the Jäderin method of using long wires suspended in the air ; the results were satisfactory, and the apparatus very portable, but temperature corrections were still troublesome, even if two wires of different coefficients of expansion were used. The invention of invar by Dr Ch. Ed. Guillaume, of the International Bureau of Weights and Measures at Sèvres, has simplified the problem very much.

Wire or tape made from invar has a coefficient of expansion almost incredibly small. The latest tape has, as certified by the National Physical Laboratory, a thermal expansion of less than 1×10^{-7} per degree C. Thus, in actual use the temperature correction may be neglected.

On the Continent invar wires are used with a piece of invar at each end to carry divisions, but English and Colonial surveyors prefer tapes as the divisions can be directly engraved. In using invar tapes in the field two methods may be employed—

1. The tape is supported on a flat surface throughout its whole length.
2. The tape is suspended in the air by silk cords passing over pulleys and carrying weights.

The latter plan is generally used. Many methods have been adopted for transferring the mark on the tape to the ground. I have tried—

1. Plumb-bob, a weight suspended by silk cord or wire.
2. Optical Plumb-bob, a telescope, which shows, in the field of view, both the divisions on the tape and the mark on the ground.
3. The Repsoldt Plumb-rod.

After very carefully examining all three systems I have come to the conclusion that the last is by far the best and most reliable.

Almost all ordinary steel tapes have etched divisions, as this method is by far the least expensive. In order to divide invar tapes with the utmost accuracy, they are placed on a large Comparator 10 feet long, and are marked every 10 feet or 3 metres with a very fine line. Microscopes are used for resetting for each division. This method is expensive, but is most accurate and reliable.

Where accurate measurements are important it is necessary to counteract or compensate in some way any molecular change which may take place in the course of time. All metals change in the the course of time, and invar is no exception to the rule. There are two methods of artificially ageing invar tapes, *i.e.* (1) Thermal treatment ; (2) Physical treatment. The thermal treatment takes six weeks. The physical treatment can be completed in a day, but it is not easy to carry out, and requires careful supervision.

In conclusion, I may say that invar tapes are superseding the older methods of measuring base lines, and we hope to have two very accurate base lines laid down in England, specially for standardising.

FRIDAY, JUNE 2nd.

SECTION I.

LORD BLYTHSWOOD IN THE CHAIR.

A SIMPLE PATTERN OF MICHELSON INTERFEROMETER.

By HERBERT STANSFIELD, B.Sc., Associate of the Royal College of Science, London,
Honorary Research Fellow of the University of Manchester.

PROFESSOR MICHELSON's interferometers are well known in connection with his optical researches, a collected account of which has recently been published,[1] but it seems to the writer that they have been left too much in the category of instruments for special investigations, and that it has not generally been recognised how readily an interferometer may be constructed, and how useful it is for educational work.

An interferometer constructed for making measurements with the highest degree of accuracy will necessarily be costly if the surfaces of the mirrors, mechanical slide, and screw are prepared by hand grinding, but a few laboratories in this country now have much less costly instruments made with plate-glass mirrors and simple mechanical construction, which are very serviceable and deserve to be more numerous than they are at present.

The instrument described below was constructed a few years ago for the City and Guilds of London Technical College, Finsbury, and is exhibited through the kindness of Professor Sylvanus Thompson.

Optical Arrangement.—The arrangement of mirrors shown in Fig. 1 is the most generally useful form of interferometer employed by Prof. Michelson.

A beam of light from the source S, rendered parallel by the field lens L, falls on the half-silvered mirror H, and is divided by the thin film of silver into two beams, one being transmitted and the other reflected. The mirrors G and F, which have polished silver films on their front surfaces, return the two beams, and if the light returned from G is, after reflection by the half-silvered mirror, very nearly parallel to the light from F which has been transmitted by the half silvered mirror, interference bands will be seen by an observer whose eye is situated at any point K in the common path of the two beams, if also their separate paths measured in wave-lengths have been nearly equal.

As the thin silver film has to be supported on one side by a plate of glass, a compensating plate C of equal thickness is placed on the other side, in order that the two beams may traverse equal paths through glass.

The observer at K sees the surface of F through the mirror H and another surface, which is the reflected image of G in the mirror H, and the interference bands, which are brightly coloured when white light is employed, are seen like the interference bands of a thin film crossing these surfaces which very closely coincide with one another when the separate paths of the two beams are made equal.

When the separate paths are not equal, and the surfaces are separated by more than a few wave-

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[1] *Light-Waves and their Uses*, by Prof. Michelson, University of Chicago Press, 1903.

lengths, monochromatic light is required; and as the distance is increased the character of the bands gradually changes, until, instead of showing a pattern that indicates the local irregularities of the surfaces, they become a system of concentric circles, and appear to be situated far behind the surfaces.

The interference bands given by this interferometer are similar to those given by any arrangement for producing interference with thick or thin films; its special feature is that the two interfering beams traverse quite separate paths of considerable length.

Glass for the Mirrors.—A little care should be exercised in selecting plate glass for the mirrors, as otherwise, when the instrument is set up, the coloured bands may be narrow and perhaps irregular, and occupy only a small part of the field which should be filled by them. A piece of plate glass may be tested by placing a small accurately worked plane glass surface upon it, and observing the interference bands, given by the thin film of air between the surfaces. A piece of glass whose surface is regular,
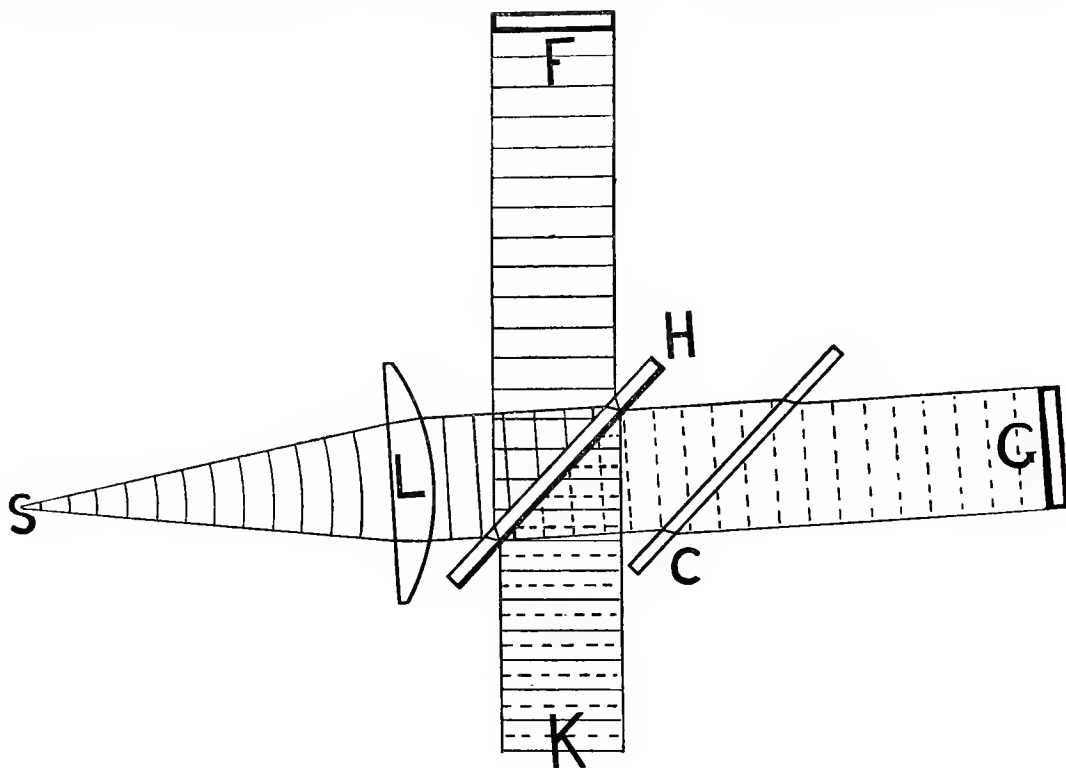

Fig. 1.

even though slightly concave or convex, is suitable for the end mirrors, if the curvature is nearly constant over a piece large enough to give two mirrors each say 2 inches square; the end mirrors made in this way from the same surface will work well together for giving the colours of thin films.

It is more difficult to find a piece of glass suitable for the half-silvered mirror, because, if for example the surface has a slight spherical or cylindrical curvature, one of the two interfering beams is reflected on the convex, and the other on the concave side of the surface, so that the curvature in this case is magnified, instead of being compensated, as it is in the case of the end mirrors. The interference pattern given by the combination of mirrors forming the interferometer generally closely resembles the pattern obtained when the half-silvered mirror surface was tested with the standard plane glass. It is not necessary that the two surfaces of the glass selected for the half-silvered mirror should be exactly parallel. In order that the field of view, when the observer's eye is close to the instrument,

shall be limited by the size of the end mirrors, and not be cut off by the distant image of the half-silvered surface, this mirror should be about 4 inches high and 6 inches long for end mirrors 2 inches square. The compensator may be cut from the same piece of glass so as to have about the same thickness, and the same composition; it does not have much influence on the shape of the bands.

Mechanical Construction.—Fig. 2 is a plan of the instrument showing the mirrors, the small castings which carry them, and the various adjusting screws. The glasses are cemented on to the castings with Chatterton's compound (a cement employed by electric wiremen), and the castings are geometrically
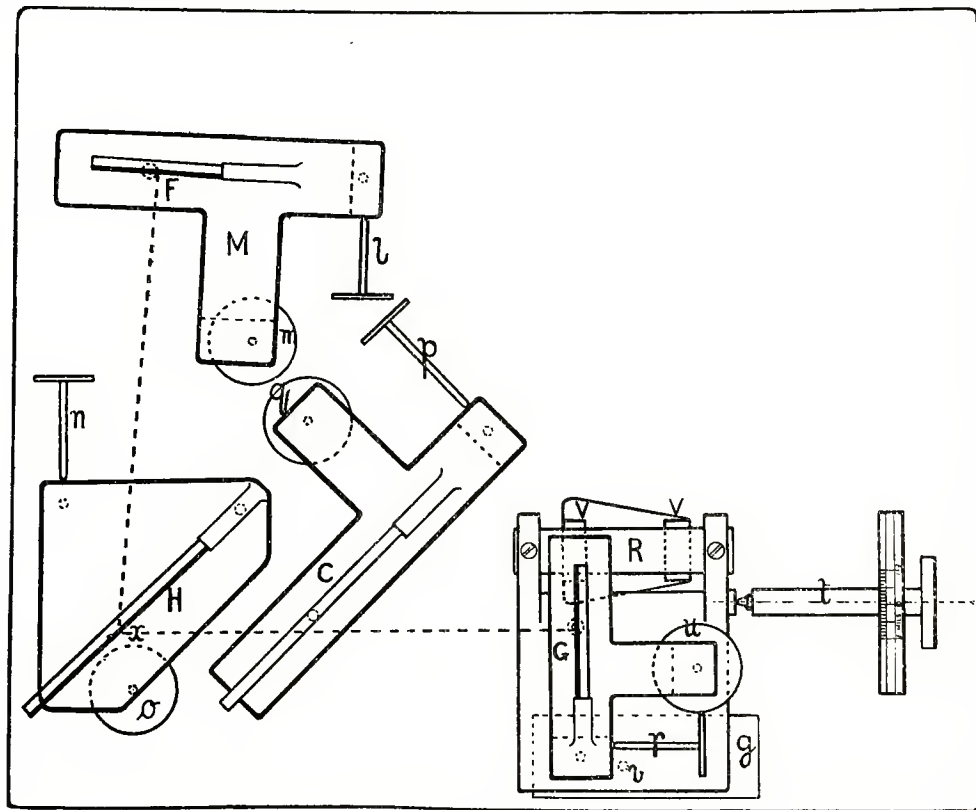

Fig. 2.

supported above the base plate. As an example of the geometrical arrangement the casting for the mirror F and its supports are shown in plan in Fig. 3, together with sketches of the separate parts. I learnt this excellent method of mounting mirrors from Professor Boys at the Royal College of Science. M is the casting to which the mirror is attached; P is a pillar whose top is filed into a triangular pyramid with blunt edges; Q has a vertical screw *n*, which fits tightly and is seldom turned, and a horizontal screw *l*; N has a vertical screw *m*.

The screws *m* and *l*, intended for fine adjustments, have $\frac{3}{32}$-inch Whitworth threads and brass disc heads. The point of the pillar P is received by a $\frac{3}{16}$-inch hole drilled into the under side of M, and gives three points of contact where the blunt edges of the triangular pyramid meet the circular edge of the hole. The other three points of contact necessary to determine the position of the casting are its points of contact with the ends of the three screws *l*, *m*, and *n*, which make contact against filed surfaces. Five out of the six contacts are secured by the weight of the casting; this is a satisfactory control, as it gives constant forces at the points of contact. An elastic band *e* is employed to keep the casting in

contact with the horizontal screw *l*.   The small drill-hole *o* helps to show where the mirror should be cemented on to the casting, in order that the vertical axis of rotation may lie in the reflecting surface.

The half-silvered mirror and the compensator are provided with angular adjustments in just the same way, and the other end mirror G has also the same angular adjustments, but the supports are carried by the table casting (Fig. 2), which has itself a linear adjustment.   In this instrument a turned steel rod R (Fig. 2) has been employed to give an accurate straight-line motion ; the ends of the rod are clamped into sockets in the table casting, and it rests in two V's in a small casting VV, screwed down to the base plate.   These V's have four points of contact with the cylinder, and another point of contact is given by a leg *v*, underneath the table, resting on a horizontal piece of plate glass *g*, which is cemented down to
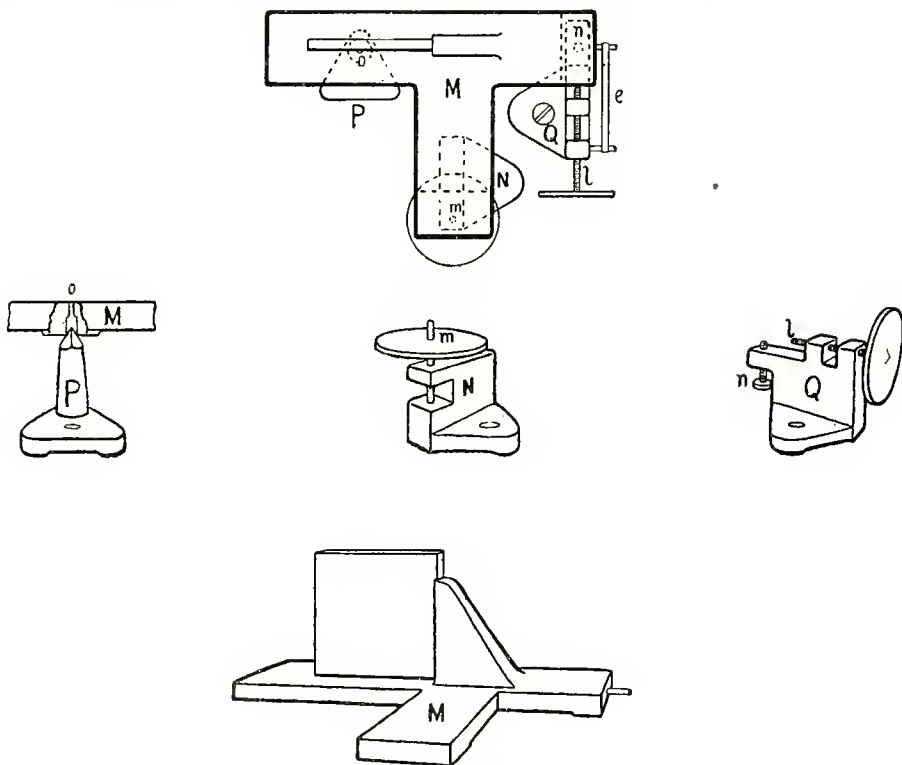

Fig. 3.

the base plate ; these five points allow the table a straight-line motion which is determined by the sixth point of contact against the end of the micrometer screw *t*.

The screw has fifty threads to the inch, and the aluminium head is divided into 200 parts, which correspond therefore to ten-thousandths of an inch ; the head is numbered in mils and so is the straight scale on glass above the head, for reading the whole turns of the screw.

Elastic bands are required to keep the table in contact with the end of the micrometer screw, and special care has to be taken to make sure that they are strong enough to secure this contact, and that they do not interfere with the other contacts by pulling in a wrong direction or position.

The cast-iron base plate is about 16 inches by 13 inches, with a top $\frac{1}{2}$ inch thick and ribs underneath $1\frac{1}{2}$ inch deep.

Silvering the Mirrors.—For the thick deposit of silver on the end mirrors I employ the method described by Wadsworth,[1] supporting the mirror with its face down in the silvering solution, and

[1] *Astrophysical Journal*, vol. i., 1895, p. 252.

watching the process through the upper surface of the glass, which is not covered with the solution. A film of silver thick enough to be quite opaque may be obtained with the first lot of silvering solution; but if, on lifting it out, a light can be seen through it, the film may be thickened by transferring the mirror to a second lot of solution. The film obtained by this method has a good reflecting surface, but there is always a slight bloom on it, which is removed when it is dry by polishing with a trace of rouge on a wash-leather rubber, and the mirror has then a beautiful reflecting surface.

The thickness of the silver deposit depends partly on the strength of the solution, and to obtain the film for the half-silvered mirror the solution may be diluted to half strength. The difficulty in making a good half-silvered mirror is to secure a uniform deposit all over the surface. I find that the silver is deposited much more uniformly over the surface if the temperature of the glass and solution is 20° C. than it is if the operation is carried out at 15° C. It is not difficult, after a little experience, to judge when the silver film is the right thickness by the colour of the blue light reflected from it, while it is in the liquid; when it has been taken out the intensities of the reflected and transmitted light may be compared by placing the mirror, as shown by an end view in Fig. 4, on a black surface, and looking at the images of two small pieces of white paper, one in front and the other behind the mirror. The observer should compare the brightness of the image produced by the beam 1 with that part of the image produced by 2, which is not overlapped by 3.
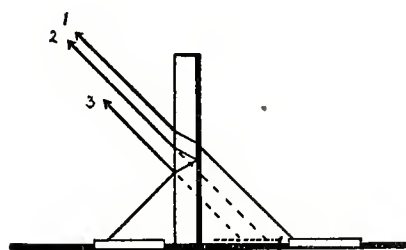

Fig. 4

Protecting the Silvered Surfaces.—The thick silver films are not very liable to injury and may be kept in good condition by polishing, but the thin film on the half-silvered mirror rubs off with the least touch. It may be protected by varnishing with a thin film of celluloid; the celluloid film gives interference colours on its own account, but they may be made very broad, and are not very noticable in the projected hands. The end mirrors may be protected in the same way, and in this case the interference colours are faint, as much less light is reflected from the front side of the celluloid than from the side in contact with the polished silver surface.

Setting up the Apparatus.—After the mirrors have been cemented on to their castings and placed in position a few general adjustments should be made. The end mirrors are adjusted by their screws so that they face towards the centre of the half-silvered mirror. This can readily be done by taking the end mirrors one at a time, and looking through the middle of the half-silvered surface at its reflection, and the reflection of one's eye, in an end mirror.

The mirror H is then adjusted until the reflected image of the mirror G appears to coincide with the mirror F. The compensating glass C may then be put in position, and adjusted to be parallel to H with the help of a distant source of light.

To obtain Interference Bands with Sodium Light.—A Bunsen burner, with a salted piece of asbestos or other arrangement for giving a sodium flame, is placed at S (Fig. 1), and a candle or other source of white light is placed behind it on the axis of the lens. The lengths of the two paths from the half-silvered mirror to the two end mirrors are now made equal; a slight mark, x (Fig. 2), is made near the middle of the top edge of the half-silvered surfaces, and the perpendicular distances from this point to the end mirrors, represented by the two dotted lines in Fig. 2, are measured and compared by means of a scale or a pair of dividers. It is difficult to make this comparison very exactly, as it is a case of comparing an inside with an outside measurement, but an error of one or two hundredths of an inch will not be likely to prevent the sodium bands from being seen, and it is possible to find them, even if the mirror G is one tenth of an inch from its proper position.

It is now only necessary to adjust one of the mirrors so that the two beams of light which are to interfere have the same direction.

The lens L is covered with a card having a pin-hole in it, and an observer at K (Fig. 1), will see a number of images of the pin-hole, if the candle is moved into a position in which they are brightly illuminated. By moving one of the mirrors away from its points of contract, it is easy to distinguish the images that are produced by reflection at G from those coming from F. If the source of light is bright, quite a number of images may be seen in each group owing to multiple reflections in the half-silvered mirror, but there is one much brighter than the others coming from G, and two bright images from F. The second image from F corresponds to the ray marked 3, in Fig. 4, and it may actually be brighter than the image produced by the ray reflected from the silvered surface, if the silver film is considerably thinner than it ought to be, but the two images may always be identified by their position. In the apparatus here described the image which is employed is the right-hand member of the pair as seen by the observer, and this holds even if the observer changes places with the light; but if the compensating glass had been mounted between H and F, the half-silver film would have been on the other side of H, and the images would have changed places. The adjustment for making the principal image from G coincide with the selected image from F may be made by adjusting the direction of either of the three mirrors, as it should only require a slight movement if the apparatus has been roughly adjusted before, as described above. The superposition of the images is generally done well enough with unaided vision, but it can be done much more accurately with the help of an observing telescope; if the telescope gives a reversed picture, this must be borne in mind in selecting the proper image from F. If the obstruction is now removed from the field lens, and the lamp moved to one side, an observer whose eye is placed at K will now probably see the field of view crossed by interference bands, which are not likely to be broad unless a telescope has been used; but in any case the final adjustment in order to obtain the broadest bands that the surfaces will give can easily be made as soon as any bands, even if very narrow ones, have been seen.

Finding the Coloured Bands.—The sodium bands when first obtained will not be very strong and clear unless the equalising of the paths has been very exact, and it will be found that they may be made clearer by screwing the mirror G along one way or the other by means of the micrometer screw *t*. During this process hundreds of bands will move in succession across the field of view, becoming more and more distinct, and then gradually fading out again until they become almost or quite invisible. This process may be repeated again and again, but it will be found that the bands are on the whole improving, that each maximum is better than the one before. These periodic changes in "visibility" have been elaborately investigated by Prof. Michelson, and are described in his published lectures quoted above. In this case the effect is produced by the superposition of two systems of bands of slightly differing width produced by the light corresponding to the two D lines of sodium; and if readings are made on the micrometer head for successive positions of minimum visibility it will be found that they come at regular intervals of about $11\frac{1}{2}$ mils., and in screwing the mirror along this distance the number of bands that cross the field of view is about a thousand, indicating a difference of one part in a thousand in the wavelengths of the two D lines. Calling the bands between one minimum and the next a "Group of Bands," one has next to try to decide which group has the greatest visibility, as, when the centre of the most visible group crosses the mirrors, the paths are equal, and the coloured bands may be obtained.

The bands in this central group appear to be in focus on the end mirrors, but in the groups which are several places from the central one there is a conspicuous parallax effect between the bands and a mark on one of the end mirrors as the observer's head is moved about. In order that the coloured bands may be seen when they come into the field of view, the candle should be moved into such a position, that part of the field is illuminated by candle-light while the sodium bands can still be seen in other parts; by watching the movement of the sodium bands one can regulate the rate of turning the screw so that there is no risk of missing a dozen coloured bands, and when at last they do come they advance across the bright parts of the field and are very conspicuous. If the group at first selected does not yield the coloured bands, then they will probably be found in the centre of the next group to it on one side or the other. Under favourable conditions it should not take more than ten minutes or a

quarter of an hour to find the coloured bands after the sodium bands have been seen. When once the reading of the micrometer screw for the coloured bands is known it becomes a comparatively easy matter to find them another time.

Circular Bands with Sodium Light.—The change from a system of bands indicating the curvature of the reflecting surfaces to a system of concentric circles takes place gradually, and is also affected by the position of the observer. Close to the instrument the bands in the tenth group from the central one may appear circular, but on stepping back a few feet from it the original pattern may again be seen.

Employing a yellow Bunsen burner flame as the source of light, I find with this apparatus that the bands become very faint after twenty groups have crossed the field of view—that is, when the mirror G has been moved about a fifth of an inch from its central position.

Circular Bands with White Light.—If the compensating glass is removed, circular bands at infinity may be obtained with white light by screwing back the end mirror G through a distance equal to the number of wave-lengths that the light was previously retarded by passing through the glass. As this retardation is not the same for light of different wave-lengths, the position for obtaining the best blue bands is not the same as the best position for red bands, with the result that the colours are nowhere so bright as in the central bands with the ordinary arrangement, but on the other hand a very large number may be seen. With a $\frac{1}{4}$-inch plate glass half-silvered mirror 450 coloured bands were visible when they were obtained in this way.

Projecting the Bands.—The ordinary coloured bands may be exhibited for lecture purposes by projecting them on to a screen by means of a beam of light from an arc lamp in an optical lantern.

The beam falling on the half-silvered mirror should be parallel or slightly diverging, and one of the condensing lenses may be taken out to avoid the risk of dangerous heating through the arc being unusually near to the condenser.

I generally focus an image of the end mirrors about one foot or eighteen inches square on a piece of white card, using a lens about 12-inch focal length.

There are often one or two stray beams of light to be screened off, and it is sometimes convenient to use a screen with a small hole in it in the focal plane of the focussing lens ; the position of the arc is adjusted so that its images produced by the two interfering beams fall in the hole, while stray light is stopped. If the half-silvered piece of glass is sufficiently prismatic, the extra beam of light from the end mirror F may also be stopped by this screen.

Lecture and Laboratory Experiments.—The apparatus may be used in a lecture to show that one beam of light can be superposed on another in such a way as to produce darkness in some parts of the common path.

The two patches of light produced by the two beams are first shown separated from one another on the screen by moving one of the end mirrors ; as they are brought nearly into coincidence the illuminations are at first added in the ordinary way, but when the carriage touches its stops the bands appear, and the illumination is reduced to darkness in one or two of the central bands.

When the bands are projected it is easy to show that the cast-iron base plate, although designed for stiffness, can be sensibly bent by a small force applied by the hand, and that it springs back at once when the force is removed ; the distortion of the base plate by local heating may also be shown by holding a lighted match for a few seconds under one of the ribs. Experiments may be made to illustrate the effect of changes in temperature of the air on the velocity of light passing through it. If the flame of a taper is put into either of the paths where the two beams of light are separated, it will be seen to wipe out the bands, although they are unaffected if it is placed in the common path ; even the hot end of a match disturbs the bands, and if it is introduced in front of the end mirror G, it can be shown that heating the air produces the same effect as shortening the path, thus demonstrating that the light travels faster in the rarer medium, and not slower, as was assumed in the corpuscular theory of light.

If a few drops of ether are poured on to a piece of blotting-paper held above and a little in front of

one of the end mirrors, violent contortions of the bands will be seen as the heavy vapour streams down across the path of the light.

In the laboratory a student may measure the wave-length of sodium light by slowly turning the micrometer screw and counting the bands as they cross a mark on one of the end mirrors. In order to obtain a fairly accurate value sets of a hundred bands should be counted, and the measurements should be made at intervals over a whole turn of the screw; the student will soon find that he is employing waves of light to indicate the peculiarities of the action of the screw. The percentage difference between the wave-lengths of the two D lines may be found with greater accuracy as it does not depend on the screw.

THE CHAIRMAN had used an instrument like this, made in his own laboratory, for testing the screw which cut the gratings that he was exhibiting, and found it exceedingly useful, because one could refer it to a definite point within something like 100,000th of an inch, and get it back again to the same position. The temperature had to be considered a great deal, and it took a very long time; but still it was probably the most accurate method that could be devised. He appreciated, therefore, all the difficulties that had been so admirably overcome by Mr Stansfield.

Dr GLAZEBROOK congratulated the author on having reduced this instrument to so simple a form. It was originally devised by Michelson some years ago, and it was very valuable to have all the details worked out so carefully and successfully, and to have the instrument in so very practicable and handy a form. It would be of very real use in many enquiries, and it was especially desirable to be able to illustrate to classes the fundamental properties that are so well shown by means of this apparatus.

Mr THORPE.—This instrument was undoubtedly very wonderful, and enabled one to test extremely carefully very minute deflections. Michelson had shown that even a difference of 100,000 wave-lengths of the cadmium line can be measured very easily by this type of instrument, and that this might be used as a standard of length, because it can always be easily reproduced. You could move out the back mirror for something like six or eight inches, or even more, and still get this particular line continually recurring on the screen. He congratulated Mr Stansfield on the excellent demonstration he had given on his very simple apparatus of a means of arriving at these interesting results.

Mr RAYNER had been very interested in this paper, as some time ago he was preparing a popular lecture, and wished to show some simple method by which two lights would produce darkness.

The Michelson interferometer seemed to be as good as any, as the experiments detailed by Mr Stansfield could also be shown. The mounting of the half-silvered mirror, the fixed fully-silvered mirror, and the compensating piece, could be very satisfactorily done on the principle of the hole, slot, and plane. Each mirror was fixed on a little brass stand which has three supports, two being fixed and the other being a screw giving the tilting motion in the direction required. The hole, slot, and planes were made of discs cut from a brass rod, and they may be fixed to a plate glass table by Canada Balsam or other cement. The other fully-silvered mirror could slide on a brass stand, on the plate glass parallel to its own plane, one of its three legs being a screw to give it a tilting motion. It also required a turning motion about a vertical axis and a guide such as Mr Stansfield uses in the form of a cylinder. A strip of plate glass on edge, against which the brass stand would bear at two points would prove satisfactory. One of these, being a screw, would give the requisite motion about a vertical axis. It would be understood that these details were not supposed to be equal to those which Mr Stansfield has described, but were sufficient to make a satisfactory home-made demonstration apparatus.

Professor S. P. THOMPSON had had the opportunity of working with this identical instrument, and found it an essentially practical one and one of very easy adjustment. The first time the trouble

was to find the adjustment that will give the rings on the screen. That, however, was not a difficult matter if one followed Mr Stansfield's instructions. "One begins with sodium light and adjusts by actual measurement of distances with a pair of compasses, and gets an adjustment which is sufficiently good to show the rings, and after that all the rest is a mere question of moving the screws until one gets to the particular stage at which one wishes to arrive. It is a very beautiful instrument for showing, amongst other things, the way in which the doubling of the sodium line interferes with definition. One gets a perfectly well defined set of rings, and with the adjustment, ring after ring disappears into the middle, until the region is reached where the different D lines overlap each other, and the rings become indistinct. One goes on displacing the mirror and once more one gets the distinctness as the overlapping region is passed." The question was put as to how one could in the simplest way make it evident to every one that there was such a thing as interference by two lights producing darkness. One did not need Mr Stansfield's instrument to show that, nor even the particularly simplified form Mr Rayner described. A very much simpler thing was to take a bit of very thin mica and look at it in the light of a sodium flame, and the bands could not by any possibility be explained except by understanding how interference can take place. The particular adjustments Mr Stansfield had introduced into the instrument were extremely good. The good geometrical principles he had followed out in the design of the various bits, and the simple way in which he had carried them out, with no unnecessary workmanship, but with excellent workmanship at the particular points at which good workmanship was required, were certainly deserving of all praise. Instrument-makers should examine this apparatus and see how much cost might be saved to those who elect to provide themselves with demonstration or research instruments of this kind, if only the makers would be content to put the workmanship where it was wanted and not to put it where it was not wanted.

AN INTERFERENCE APPARATUS FOR THE CALIBRATION OF EXTENSOMETERS.

By J. Morrow, M.Sc., M. Eng., University College, Bristol, and Professor E. L. Watkin, M.A., Hartley University College, Southampton.

Having recently undertaken some research work on the elasticity of metals which involved the use of extensometers of considerable delicacy, the authors found some difficulty in determining the constants of the instruments with sufficient accuracy, since any mechanical device with which they could be compared would necessarily be liable to defects of the same kind and order of magnitude as those which it was desired to detect. They therefore decided to calibrate these instruments by interference methods, and after some preliminary trials the apparatus here described was constructed and found to fulfil all requirements.

The great advantage of comparison of displacements with the wave-length of monochromatic light by an interference method is this, that the measurements may be made directly in terms of wave-lengths, and do not depend on those of the components of the instruments used.

Description of the Apparatus.—In Figs. 1 and 2 ff is a rigid cast-iron frame, into the top of which is screwed a steel tube a, the lower portion forming guides gg, between which slides a gun-metal sleeve c. A second steel tube b is attached to c by a set-screw, and the whole can be raised or lowered by the fine threaded screw s, actuated by the levers ll.

The extensometer to be tested is attached by means of its gauge-screws to a and b as to an ordinary test piece, so that the displacement to be measured is that of b relatively to a, and this is effected as follows :—

To the lower end of a and at right angles to its axis is attached a thin piece of optically plane glass p, whilst b carries at its upper extremity, nearly in contact with p, a mirror q of black glass and small curvature. This mirror is supported by the levelling table r, by means of which its centre of curvature is placed accurately on the axis of the tubes. Light of known wave-length passing through a hole in a is directed to this optical system by a piece of plane glass set at an angle of 45 degrees to the axis; and the interference rings thus produced are viewed through the microscope k which slides in the upper tube. When the screw s is turned, b is raised or lowered, and a ring appears or disappears for every half wave-length that b is displaced.

In the eye-piece of the microscope are three cross-wires, one central and at right angles to the other two, both extremities of a ring being thus under observation.

The displacement is produced by turning the lever l which is pivoted independently of the remainder of the apparatus, so that vertical pressure on it is not transmitted to l and b. The sleeve c is kept in contact with two worked surfaces on g by the piece h pressed against it by the springs ee, and at the same time contact with the point of the screw s is ensured by the springs dd.
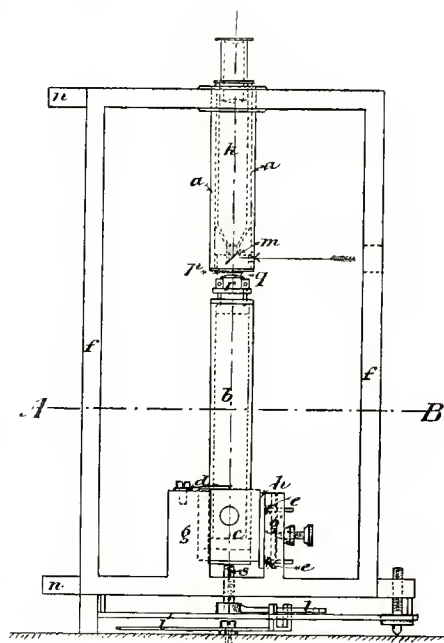
To attach an extensometer, the piece b can be lowered in its sleeve by releasing the set screw; the ends of the instrument can then be passed between p and q. Where this is not possible, the springs d are turned outwards, so that the tube b can be removed altogether.

When the extensometer is one to be used in a horizontal position, the calibrator can be laid on its side, resting on the legs nn.

The light used was that from a sodium flame brought to a focus at p by a condensing lens: with this light each new ring corresponds to a distance of $29\cdot46 \times 10^{-6}$ cms.

The tilting table r carrying the mirror is set so that the centre of the system of rings is at the centre of the field of the microscope, $i.e.$ on the axis of the tubes, and no displacement of the system as a whole occurs when b is raised or lowered. This is an important point, and has of course to be very carefully attended to; but when once the adjustment has been made, the instrument is found to work very satisfactorily, and the centre of the rings remains quite stationary during a whole series of observations.

— Elevation.—
— Fig. 1.—

— Sectional Plan A-B. —
—Fig. 2.—

With this adjustment, and using a low-power microscope, the measurement is effected by watching the motion of the third ring from the centre, the separation and definition of the rings being sufficiently good to enable one to estimate $\frac{1}{50}$th of the distance between two rings fairly correctly, and the calibrator readings are taken to be correct to this degree of accuracy. As the possible range of displacement is well over that corresponding to the shift of 100 rings past the cross-wires, the final determination
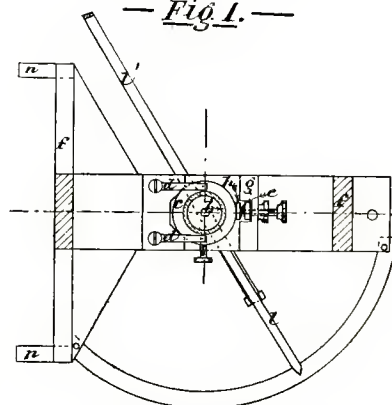
of the extensometer constants should be correct to at least 1 part in 1000, the order of accuracy desired.

It was at first intended to place a fine scale in the microscope eye-piece, but this was found to be unnecessary. It is to be noticed that an instrument of this character is quite free from " backlash," a possible defect for which it was especially desired to test the extensometers. In addition, the apparatus is easily set up for use, and not liable to be disturbed by external causes. Should such disturbance occur, both calibrator and extensometer are equally affected.

Test of an Extensometer.—The points to be examined in calibrating an extensometer are :—

(i.) The value of the magnification constant.

(ii.) The action of the instrument at starting and reversing (in order to discover whether there is any " lost motion ").

(iii.) Within what limits the readings are proportional to the displacement, and what correction is to be applied when this is not the case.

The following tests will serve as examples :—

The extensometer was of the differential mirror type, arranged so that a displacement of 10^{-6} cms. could just be detected. In the first place simultaneous readings were taken for a series of displacements in the same direction. The test commenced with the extremities of a diameter of a ring (the third from the centre), at the cross-wires of the microscope. A displacement of exactly ten rings was then produced and the extensometer reading noted. The process being continued, the following Table was obtained :—

TABLE I.

CALIBRATOR.		EXTENSOMETER.
No. of Rings.	Reading.	Differences in $\frac{1}{40}$ inch.
0	50·0	
		14·5
10	64·5	
		14·6
20	79·1	
		14·3
30	93·4	
		14·4
40	107·8	
		14·4
50	122·2	
		14·4
60	136·6	
		14·4
70	151·0	
Mean difference for ten rings		14·43

The mean difference of 14·43 corresponds to a displacement of $29·46 \times 10^{-6}$ cms. The magnification of the extensometer was therefore 3012.

On another occasion the instrument was arranged to give a difference of 13·75 units for a displacement of ten rings, and was then tested for accuracy in reversing. Readings were taken for a displacement of five rings, followed by a return to the original position. Three such series are given in Table II.

TABLE II.

CALIBRATOR.			EXTENSOMETER.				
No. of Rings.	Reading.	Difference.	Reading.	Difference.	Reading.	Difference.
0	163·3		119·9		120·0	
		6·8		7·0		6·8
5	170·1		112·9		113·2	
		6·9		6·9		6·9
0	163·2		119·8		120·1	

It will thus be seen that the instrument was free from defect within the range tested.

Mr J. Morrow wished to add a few words, although Mr Watkin had touched on all the points. The difficulty of calibrating extensometers which read to the nearest millionth part of an inch was well known. The accuracy of such an instrument had to be tested in two ways: it was necessary to know its magnification constant and to show that there was no backlash. He (Mr Morrow) regarded the second as being even more important than the first since the backlash error would come into the readings if at any time during the test a portion of the load were removed and the extension diminished. Any mechanical device used for calibrating such an instrument was likely to have this same backlash defect; the use of interference rings, however, abolished the difficulty. Interference rings have been used before for experimental and physical measurements, but he believed that this was the first time they had been applied to engineering purposes. They (Messrs Watkin & Morrow) had tried a thallium light instead of a sodium light, at Professor Thompson's suggestion, but they had not hitherto been successful, although they were now on the way to obtaining a good thallium light by using the thallium salts in the flame. They had recently improved the sodium flame, and now thought it a very good one; in a room free from draught it gave a very steady and intense light.

The Chairman expressed the thanks of the meeting to the authors for their paper. The difficulty of these delicate measurements was well known, and he knew of no other way in which the rings could be relied upon. What Professor Thompson had pointed out as to the double line of sodium was of course always present with sodium, but there is always a considerable range free from interference. The extensometer seemed to be very simple, and that was always an important matter with such instruments.

THE PRESENT POSITION OF EDUCATION IN OPTICS

By Dr R. Mullineux Walmsley, F.R.S.E.

The meeting of the first Optical Convention which has been called together in the British Isles appears to the writer to be an occasion upon which the members, who must, as members, be supposed to be interested in the development of the science of optics in its many varied aspects, may be asked to turn aside for a time from scientific problems and matters interesting to the trade to consider the far-reaching and fundamental problems in education with which the future of the trade is so intimately associated.

The subject of education specially directed towards the training of those connected with the trade or mystery of optics, from the humblest workman at the bench, to the most advanced investigator into the most abstruse scientific problems which yet remain unsolved, is one which has attracted increasing attention of late years, and nowhere more so than in the building in which we are assembled. The writer therefore hopes that a preliminary historical sketch of what has been done in recent years will not be out of place, and will not prove tedious to his hearers.

The first real attempt to improve the scientific position of the optician was made by the British Optical Association, when it founded its scheme of examination and certification in 1895. The educational requirements of this scheme, however, could only be met at the time by private coaching classes, and it did not lead to any systematic educational courses, and moreover was limited to one, though an important, branch of the trade.

When the Worshipful Company of Spectacle-Makers, waking up from its long sleep, became alive to the fact that there was in existence a trade which was not remotely connected with the title which it bears, the subject of the education of the optician claimed its attention not less than that of the

certification of those more directly connected with the special branch of the trade connoted by the words "Spectacle-Makers." The limited resources at the disposal of the Company, however, were quite inadequate for the task of initiating a comprehensive scheme of optical education, though it is open to question whether these resources, if co-ordinated with public funds, which were at that time being devoted to technical education, would not have formed a nucleus round which ere this a really substantial scheme might have been developed, and the future of optical education assured. Beyond, however, assisting with a loan of apparatus, which is still maintained, contributing thirty and fifty guineas in two successive years towards the salaries of instructors of optical classes at the Northampton Institute—a grant which was then withdrawn—and appointing an "official instructor" (whose official duties, *qua* instructor, have not been very onerous), the Worshipful Company has been content to act indirectly by means of its "certification scheme." That this scheme, the merits or demerits of which are not now under discussion, has not had much effect upon real optical education is an open secret. Educationally, its greatest product has been a certain amount of private coaching to enable the candidates to satisfy the examiners. Most of the candidates have been unable or unwilling to attend systematic courses of instruction, including lecture, laboratory, and workshop work, because such courses were only available in London. This part of the subject, however, will be referred to more fully later on.

At the very commencement of the work of the Northampton Institute, which was opened in 1896, it was the intention of those responsible for the development of the technical courses that instruction in technical optics should form, as soon as possible, a marked feature of the classes. The Principal, some years earlier, had been associated with Dr S. P. Thompson at the Finsbury Technical College, and had there not only come into contact with the curvature method of teaching geometrical optics—a method which he has always used since—but also had assisted at an attempt to found such technical classes, an attempt which had to be abandoned for want of space and the necessary laboratory resources. It is, therefore, but natural that, planning the work to be carried out at the new technical Institute in Clerkenwell, the ancient home of the optical trade in England, he should have reverted to the plans previously abandoned, and determined to make another attempt under better auspices and with larger resources available. The matter, however, could not be taken up immediately, as it was necessary first to get into working order numerous classes, laboratories, and workshops in engineering, mechanical and electrical, and in artistic crafts, for which the demand was both larger and more urgent at the moment. Moreover, the building was not ready for full occupation until the session 1897-98, although pioneer classes were held in 1896-97.

When, therefore, in the summer of 1898 the Worshipful Company of Spectacle-Makers was deliberating on the best methods of assisting the Optical industry, schemes were already in existence for the formation at the Northampton Institute in the succeeding winter of classes in technical optics, although it was not clear how much could be done with the resources available, which were not illimitable, and upon which there were already very heavy demands. In July of that year a conference was held between the governing body of the Institute and the representatives of the court of the Company, and as a result what are believed to have been the first systematic educational courses in technical optics were established in the session 1898-99. It may be of interest to place on record that the classes consisted of two lecture courses by Mr (now Dr) Drysdale on (*a*) Optical and Scientific Instruments, and (*b*) Applied Optics and Heat and its Applications, for opticians, and a lecture course on Visual Optics, by Mr Lionel Laurance. In the "Table of Special Courses," also in the "Announcements" of the Institute for the session 1898-99, there appear two courses for "Opticians," and three for "Optical, Mathematical, and Surveying Instrument Makers." These courses included laboratory and workshop work and mathematics combined with the lectures noted above. It may therefore be claimed that this first essay in optical education comprised a well-thought-out scheme for the requirements of evening students engaged in the chief branches of the trade. Towards the cost of these classes and courses the Company, as has been stated above, contributed the fee of the lecturer

on Visual Optics, and also a loan collection for laboratory use of apparatus directly connected with the last-named subject. Similar financial support was continued for the session 1899-1900, but was withdrawn in the summer of 1900; the loan collection still remains available for the laboratory work at the Institute.

The next point to note as having a direct bearing on the recent development of education in technical optics is the formation of the Optical Society in June 1899. It is not the least of the good work which this Society has done that it has kept alive the interest of the different branches of the trade in the technical optics classes which were already in existence at the Northampton Institute when it was founded. The writer considers that the moral support which this Society has given to the classes has been of the greatest value, but in addition, late in the summer of 1900, when the intimation was received from the Worshipful Company of Spectacle-Makers that the grant hitherto made would have to be discontinued, some of the members of the trade, acting through the Society, stepped into the breach, and personally subscribed the amount so withdrawn, thus saving the situation at a critical moment. This contribution to the maintenance of the classes has been continued by the trade through the Optical Society ever since, and for the last three years has been doubled. It will be admitted that this is a very substantial proof that the efforts of the governing body meet with the approval of the trade.

The Optical Society has, however, been of assistance to the classes, not only financially and in maintaining the interest of the trade in the work carried on, but also by putting the trade in a position to advise with the authorities of the Institute as to the details of that work. In October 1901 the Society appointed an Educational Committee for the purpose of enquiring into the system in operation at the Northampton Institute in relation to the classes in optical subjects held there, and for the purpose of considering and reporting to the Society on the feasibility of establishing classes in similar subjects in other parts of London and in the provinces, and further to enquire into and report upon the best methods to be adopted for the training of opticians in practical and theoretical optics generally. The Committee held many meetings throughout the winter of 1901-1902, and presented a report in June 1902. The report very favourably referred to the work which was already being done at the Northampton Institute, and contained valuable suggestions for its further development and upon the best methods of training opticians. After a review of the whole position the report concluded with a definite recommendation to the effect that the Optical Society should instruct its Council to enter into correspondence with scientific and trade societies and individuals interested in optics, with the object of forming a strong deputation from the whole of the optical trade to wait upon the Technical Education Board of the London County Council for the purpose of advocating the urgent necessity for the provision of a central opto-technical school or institute with as little delay as possible.

Whilst this Committee was sitting an important paper on "Opto-technics" was read on the 23rd April 1902 by Dr Sylvanus P. Thomson before the Society of Arts. In this paper, to which further reference will be made later on, it was very strongly urged that a real opto-technical institute should be established as early as possible, either at the Northampton Institute or elsewhere. The same conclusion had been arrived at by the Educational Committee of the Optical Society, who, in the report above referred to, asserted that "the time is now ripe for a much more ambitious scheme in which the students should be required to give the whole of their time for two or more years to a thorough grounding in optical work, both theoretical and practical, each being carried out to its highest and latest developments." It was urged by the Committee that it was only by providing such educational facilities that the optical trade of this country could be expected to compete with its foreign rivals, and it was further pointed out that if established at the Northampton Institute the more numerous teaching staff and the larger equipment available would greatly improve the usefulness of the evening courses already in existence there. The recommendation of the committee was in due time adopted by the Society, and steps were taken to form a deputation on the suggested lines to wait upon the Technical Education Board.

As a result a very strong deputation, representing all sections of the trade, was formed, and was received by the Board on the 3rd December 1902. Mr Conrad Beck was the spokesman, and put forth

the views of the trade in a speech which is too long to quote in full, and is somewhat difficult to abridge or abstract. He showed that the value of the scientific instruments imported into this country had risen from £99,000 in 1897 to £710,000 in 1901, whilst the export had remained stationary at about £250,000 per annum for five years. He then proceeded to show the extent of the ground covered by the trade, a point upon which, in view of the exhibition downstairs, and of the Catalogue thereof for this Convention, one need not further enlarge here. He pointed out that as regards new inventions it was a fact worthy of the most careful attention that the country where optical training is best, and where the optical literature is best, is the country where the largest number of optical inventions originate, namely, Germany, and expressed the opinion of the deputation that English optical education was fifty years behindhand. After a review of the defects of optical education during the preceding fifty years, and of the further defective state of optical literature in the English language, he enlarged upon the fact that the optical trade was then showing great activity in its desire to overcome the defects under which it laboured. Reciting the assistance which the trade had given to the classes at the Northampton Institute, he continued: "I hope I have said enough to convince you that the trade are in earnest; they are prepared to assist in the formation of an optico-technical institute by also giving their time. It is their opinion that the success of an institute of this kind will largely depend upon its being in very close touch with the industry," and he further pointed out the necessarily close connection between theory and practice, and the necessity for the work being directed into the right channels. He finally handed in a statement of the accommodation which such an institute should provide.

The matter brought under its notice by the deputation was enquired into and considered by the Technical Education Board, and in July 1903 the Board decided to make a grant of half the requisite salary to enable the governing body of the Northampton Institute to appoint a head of a new department of technical optics. With this assistance the work which had hitherto been carried on as a part of the work of the Applied Physics Department was separated as a distinct department, under a responsible head, and in the following September Mr S. D. Chalmers was appointed to the new position. The idea at the time in the mind of the Principal was that Mr Chalmers should prepare during the winter of 1903-4 the plans and details for the new building, that the necessary arrangements should be made, and the building operations started in the summer or autumn of 1904.

Unfortunately for the development of this work, the interview of the deputation with the Technical Education Board almost synchronised with the passing of the Education Act (1902), which recast the whole of the educational system of the country except London, the London case being dealt with separately in the Act of the following year (1903). During the whole of 1903 therefore it was known that large and far-reaching changes in the official administration of educational funds were to be brought about in the very near future, and in due course, in the spring of 1904, the Technical Education Board was superseded by the Educational Committee of the London County Council. Although a year has elapsed since that committee was formed, nothing has yet been done with respect to this particular matter. The chief reason, a reason which is known to everyone, is that the new committee is overwhelmed with an enormous mass of details connected with the work of elementary education, and that it has not therefore been possible for it to take up fully, as yet, the further development of the work bequeathed to it by the Technical Education Board.

Meanwhile, however, valuable time is being lost, and the leeway is increasing, despite the efforts to stem the current with the inadequate resources available. From the foregoing historical sketch, it is evident that on all grounds the time is now ripe—if not, indeed, over-ripe—for making another distinct step forward in the development of education in optics in this country, and the step which the writer of the paper thinks to be the one which is more immediately the obviously best one, is the founding, erecting, and equipping of a building to be specially devoted to the work, under the title of the "British Institute of Technical Optics," or some other title which may be deemed more appropriate.

With the present accommodation at the Northampton Institute, although day work has been started during the current session, the space available is inadequate for its proper development. The original

evening classes noted above have been added to and developed from time to time, and are doing valuable work of all grades, from the lowest to the highest requirements of the subject; but the demands on the available accommodation are so great that only two laboratories and a workshop can be specifically devoted to the work of the department, the lecture and other work having to share rooms in common with other departments. Notwithstanding these drawbacks, it may be legitimately claimed that the work done has met with the approval of the trade. The students are enthusiastic and diligent, and are further stimulated by the valuable prizes which for several years have been offered by the generosity of Mr James Aitchison and of the Worshipful Company of Spectacle-Makers. Moreover, though the conditions are not so favourable as they ought to be, a fair amount of research work has been produced, some of which has been, and will be I hope in the future, of direct value to the industry. I can name only a small part, *e.g.* Dr Drysdale's work on " Curvature Measurement " and on the " Design of Prism Binoculars," Mr Chalmers' work on the " Testing of the Angles of Prisms," the " Measurement of the Refractive Index of Lenses " (to be read to-morrow), and on the " Theory of Symmetrical Objectives "; Mr Bull's and Mr Jolley's work on " Trichromatic Photography " and on " Photometry." These few instances, which are far from being exhaustive, will suffice to show that this aspect of the work of an optical institute has not been overlooked.

In the new building which has been projected there will be an opportunity of developing the ideals with which the work was started as modified and extended by the experience of the last seven years. This experience—the only experience of its kind as yet available—will in the first instance be devoted to the planning and arranging of the building on the most convenient lines. The general scheme for such a building has been in existence for three or four years, if not longer, and it is very similar to the scheme handed by the deputation to the Technical Education Board as noted above. It provides lecture-rooms and class-rooms of a size estimated to meet the probable requirements of the work, and in addition, general and special laboratories and workshops. The general laboratories would consist of rooms for elementary and advanced students in optics, and ancillary physical and chemical laboratories, in which the subjects of physics and chemistry would be dealt with from the point of view of the optical student. There would also be a special chemical laboratory for problems connected with the manufacture of glass. It should not be overlooked, however, that if the new Institute were, as is proposed, in the immediate neighbourhood of the Northampton Institute, the students of it would have access to, and reap the advantages of, the well-equipped physical laboratories of the older Institute. Amongst the special laboratories would be one for telescopes and surveying instruments, another for photographic work, with the necessary dark room attached to it, a laboratory for photo-process work, another for microscopy, another for projection work, whilst in addition there would be a photometer room, a special testing-room, such as would be met with in a well-designed optical factory, and last, but not least, a sight-testing room. The workshop accommodation would consist of two glass-working workshops and a workshop for spectacle-frame-making, a furnace room for glass-working, and wood-and metal-working workshops. The above rooms, as planned, would require a floor space of about 23,000 to 25,000 ft., inclusive of the necessary departmental rooms for the head of the department, who would have his private laboratory, and for the assistants and clerks. This does not, however, include corridor, lavatory, and cloak-room space, and there is no provision for administrative work, the bulk of which can be readily carried on in the adjoining Institute.

But it is time to turn from the material requirements of buildings and equipment to the other aspects of the problems presented, not only in London, but throughout the country, and to examine, however briefly, what is necessary to place optics in an educational position not less advantageous than that which has been attained by, let us say, the engineering industries. No one is more painfully aware than the writer that these latter industries are far from being in the educational position which their importance requires; still, they are much better off than their smaller brother, the optical industry.

Dr Thompson, in the paper[1] to which reference has already been made, points out that in most

[1] See *Journal of the Society of Arts,* vol. l. (1902), pp. 518-527.

of the technical institutes, which are now to be found in every large town in the Kingdom, the equipment for teaching optics is very much behind that for teaching, say, chemistry or electricity. This defect should be remedied without delay, and in every technical institute, classes in Optics should be established, not for the purpose of passing examinations, but to provide the young opticians of the district with facilities for acquiring a knowledge of the subject on rational and up-to-date lines. Doubtless at first there would be a difficulty in finding the right kind of teachers for these classes, more especially, as unfortunately, or perhaps one should say fortunately, not even a proper text-book on the subject exists. It would be not the least of the functions of the proposed " British Institute of Technical Optics " to supply this want, and further, probably by short summer courses, to wean the teachers of physics in secondary schools from their obsolete methods of teaching elementary optics, and to put the teaching in these schools on the right lines.

In the larger towns and in the suburban metropolitan districts the technical institutes should go further, and provide special classes in technical optics, at least for the most numerous sections of the trade, the sight-testing optician, and the sellers of optical—including photographic—goods. A start has been made at the Manchester Municipal School of Technology, but there, strange to say, the classes appear under the heading of " General Science " and not under " Technology." It is fairly certain that, to avoid educational waste, such classes should not be developed so far as to take in the highest branches of the subjects, as for these in most towns and districts only one or two students would probably be found. Indeed, it is the small number of students which even the more elementary classes would attract which forms the chief obstacle to their establishment, especially where those in control worship mere numbers, or, worse still, are under the influence of the examination curse, in both cases losing sight of the proper aim of all real technical education, the benefit of the industry involved. Therefore it must not be overlooked that, although the numbers in the optical trades are far from being negligible, they cannot be compared with those in the engineering industry and its allied trades. If this fact be forgotten, there is a danger that county and borough education committees may be discouraged to the verge of abandoning the classes because they are not so numerously attended as those which meet the necessities of the larger industries.

By a well-devised system of scholarships, however, the best men in the day and evening optical classes of the provincial technical institutes could be passed forward for shortened courses in the higher branches at the London Institute, and thereby an unnecessary duplication of expensive equipment and specialist teachers would be avoided, and sound, as distinct from parsimonious, educational economy would result. All the figures available tend to show that, at least for some years to come, one such higher Institute would suffice for the needs of the country. Nay more, by concentrating these higher resources at one point, the capacity for dealing with special problems would be enhanced, and the direct benefit to the trade would be greater than if they were scattered throughout the country.

Turning now to the work of the proposed new Institute of Technical Optics, the present evening work of the Northampton Institute, for those engaged in the industry whose time is so occupied that they cannot attend day classes, would be continued and developed. Such work in its lower branches would be similar to that which should also be done in the provincial and suburban Technical Institutes. It must be remembered that the keynote of this work, as indeed must be the keynote of all such real technical work, wherever undertaken and in whatever subject, is that the teachers, in lecture-room, laboratory or workshop, must be men who have a practical and technical knowledge of the requirements of their students. The difference between the curvature method of teaching geometrical optics and the older academical methods is one which *mutatis mutandis* must be carried into every section of the work. When the Northampton Institute technical classes were first started, the relation between the trade and the schools was very much the same as was the relation between trade electricity and school or university electricity in the early sixties, when the British Association Committee on electrical standards was beginning its work. Put very crudely, it may be said that the electrical trade laboratory and workshop in those days was more scientific than the university and secondary school

lecture room and laboratory. So it was seven years ago in the optical trade in England, and the advance during the interval has made as yet but a slight impression on this anomalous condition of things. With, however, the record of the last forty years in electrical development before us, we may hope that matters optical will move even more rapidly, and that the interaction of the highest academical science and research with the requirements and progress of modern commerce will result in as happy and as fruitful a record in optical development in the next ten or twenty years.

It must be remembered that the evening classes referred to offer instruction to all grades of the industry from the lowest to the highest, *e.g.* from the making of a spectacle frame to the designing, figuring, and testing of the object-glass of a photographic camera. But throughout the practical work is ever accompanied by instruction in the principles underlying it, and in no case are hand and eye trained without some endeavour to develop the intelligence. This work would be carried forward and expanded, but its essential character would remain that which has been so successful in the past. The courses would be systematic and progressive from year to year, and the clever and ambitious youth would have ample opportunity for learning all the "secrets" of his trade or mystery.

But it is in the systematic and thorough courses of the day work that the greatest opportunities lie for the equipping of the trade for the industrial struggle with its foreign rivals in the century which is still young. The student who takes the complete course should, at the end, find himself not only well trained in all the scientific principles underlying his craft, in the details of the design of all classes of optical instruments, in practical and accurate methods of testing optical instruments for all the defects which practice and theory have proved to be possible, in the properties and limitations of the materials with which he has to work, and the methods of experimenting by which those properties may be modified and those limitations pushed further back in the future. More than all these, he should have carried through some piece of optical research and felt the pleasure of enlarging the boundaries of our knowledge of optics.

There would necessarily be an entrance examination to ensure that the student is in a position to benefit by the instruction provided. At present the obligatory subjects at the Northampton Institute are elementary mathematics and English, the former consisting of arithmetic, mensuration, algebra to simple equations, and elementary geometry. In English, the candidate's power to express himself in simple and concise language is all that is required. Optional papers are set in more advanced mathematics and in elementary science.

Following on these modest entrance requirements, the first year's course should consist, as it does now, in a thorough grounding in moderately difficult mathematics, combined with lectures and laboratory work in applied optics, optical instruments, and general physics, together with mechanical drawing and work in the optical and instrument workshops. To this it may be desirable to add in the future a special course of elementary chemistry.

The scholarship students coming up from the provinces would, if properly taught, be in a position to be excused this first year's course, and would pass direct into the second year's course, in which mathematics would be carried considerably further, and the design of optical instruments with the necessary drawing practice and calculations would bulk largely. In addition there would necessarily be a fair amount of laboratory work involving exact measurement and the adjustment of delicate instruments, and in the workshop the making of simple forms of objectives to satisfy specifications and to verify calculations made in the more theoretical classes. At least one more year's work would be required, in which mathematics would be carried to the furthest development required in optical work, and the many and varied problems which present themselves in the best optical work would be dealt with systematically. Optical theory and measurement would also be carried to their highest stages, and optical research would be undertaken by the more capable students. If found necessary, an additional year should be added to enable the programme so briefly sketched to be dealt with adequately, for until some further experience has been gained, it would be premature to limit the time in a cast-iron manner. Moreover, the scheme should be sufficiently flexible to allow honours

graduates from the Universities, who desire to specialise in technical optics, to take up the higher branches of the work, and thus qualify for positions as designers and optical engineers in manufacturing works.

Such is the scheme, which, in the briefest outline, and with the details imperfectly sketched in, I venture to place before the Optical world at this first great gathering of opticians in these islands, and I ask, Is it too ambitious? does it aim too high? Why then is it hanging fire? I have given you the history of the movement, and you will have gathered how it is that administrative difficulties, caused by educational developments remote from our subject, are blocking the way. True it is that I have an unofficial assurance that although nothing has been put on the estimates of the London County Council for new buildings for the current financial year, which ends on the 31st March 1906, the subject will be favourably considered during this year with a view to a grant being made in the following year. One is tempted to sigh, not for the State aid which is so lavishly given to our German competitors by their bureaucratic rulers, but for the even quicker methods of our Transatlantic cousins, where, I believe, that long ere this, for so clearly established a case, some public-spirited citizen would have taken the whole burden on himself.

I cannot refrain from reminding those social reformers whose souls are vexed with the complicated problem of the unemployed that every high-class optical instrument imported into this country represents a serious loss of wages to our skilled artisans. I need not remind those before me of the relative value of labour and materials, in say a £10 or £15 photographic objective, nor need I name other optical productions in this connection. But I should dearly like to emphasise it for the benefit of the social reformers to whom I have alluded.

In conclusion, may I venture to express the hope that the solution of our present difficulties may be accelerated by this Convention, and that before its successor meets—and let us hope it will have a successor—the " British Institute of Technical Optics" may be fully launched, and that both optical science and optical industry may be reaping the benefits of its work.

The CHAIRMAN.[1]—Dr Walmsley has given us a very interesting lecture, and to me one that is rather new, as far as the history of the matter is concerned. I knew, of course, that there was the object in view of increasing the teaching of optics where it is so very necessary, and I also knew how very necessary it was, as I think every one who knows anything about the trade in this country will agree. There are a few bright and brilliant examples, but a great deal is done by rule-of-thumb, and it would be better done if it were done by the law of optics. I trust, therefore, that the outcome of this admirable paper may be the fulfilment of the wishes of Dr Walmsley.

Professor S. P. THOMPSON—I almost wish, Sir, you had called on Mr Beck to speak before I made any remarks, because you would then have had the view, not of an educational authority, but of one who would speak of the needs of the optical industry as felt within the industry. However, you have called upon me, and therefore I will proceed to say how greatly I appreciate this paper of Dr Walmsley's, setting forth the efforts that have been made, in spite of difficulties, to carry on optical work in this place, and of the ideals of the scheme that he has put forward for the creation of a real Opto-Technical Institute worthy of this country. He has referred to two efforts of mine towards this end. In 1886-1887 I was trying to stir up the authorities of the City Guilds Institute to do for optics the same kind of thing they had done some five years before when they founded their Technical Institute, and started the first electro-technical laboratory in this country, or, for that matter, in the whole world, in the classes that were held under Professor Ayrton at Finsbury. I said that what had been done for the electrical industries in the creation of a technical laboratory on lines different from an academic physics laboratory in the universities might be done over again, not necessarily at Finsbury, with the greatest benefit to the optical industries. I even went so far as to draw up a scheme for an optical laboratory,

[1] Lord Blythswood.

with time-tables, with an organisation of teachers, and even naming the pieces of apparatus that were necessary in a laboratory that was to be mainly an optical testing laboratory. I put that scheme before the Committee of the City Guilds Institute. They gave it some consideration, and then they said, "Where are you going to put it? There is no room at Finsbury. You say your work there is cramped, so where are you going to put it?" I said, "You cannot put it there without enlarging the building, or making a new building." "Very well," they said, "it will have to remain where it is," and it was put on the table. Some three or four years later, when the "Beer money" came in, and the city parochial authorities were reconstructed, the Charity Commissioners held a special enquiry into the needs of technical education in London. It was known that they had something like £200,000 a year to give for the purpose of technical education. I interviewed one of the Charity Commissioners, talked to him on the subject, and gave him the very scheme I put forward to the Committee of the City Guilds Institute. I urged upon him that, when they were dotting London with technical institutes, they should locate one, preferably in Clerkenwell, because it was the centre of the metal and optical trades, to be an optical institute. I did not say there should not be other things also, but that they should make that the fundamental feature, just as at Finsbury the electro-technical work had been made the fundamental feature. I pointed out the great advantage of these technical institutes being in different parts of London—namely, that they need not be copies of one another, but each one would have its own character, developed on its own lines, and that they could be as different from one another as possible with the greatest advantage. Alas for the vanity of human wishes! The Charity Commissioners and their advisers were bitten with what I think I may reasonably call polytechnic fever. The polytechnics were to attempt everything. They were to have evening classes for every trade and profession—music classes, photographic classes, shorthand classes, French classes, and German classes; and I do not know how many more. Everything was to be polytechnic or nothing. The idea of a monotechnic institute was entirely unacceptable, and once more my scheme was rejected. In 1886 I gave a course of lectures, with some attempt at laboratory work. They were evening classes at Finsbury, and every year since then I have given short courses in one or other branches of optics until about a year ago, and then I dropped it in order that it might be perfectly well understood that this Institute at Northampton Square, which had taken up the work, was not going to be cut into or interfered with by anything I might be doing. I therefore held my hand from any sort of attempt to draw students away from the Northampton Institute. Why is it that optics, the real study of optics, has fallen so far behind in this country that it is necessary to talk in this strain about the neglect of it? Well, as I said yesterday, I believe the main responsible party is, in the first instance, the British Government, as represented by the late Science and Art Department; for it never could be brought to see that the teaching of science in these respects ought to be made technical. They were spreading science classes all over the country thirty years ago by means of payment by results to teachers. They had enormous classes in chemistry and in what they called electricity and magnetism, which was the old electricity and the old magnetism of the text-books of forty years ago—rubbing glass with silk, or playing about with permanent magnets. They had also a subject, "Sound, Light, and Heat," in which all these subjects were taught on far too academic lines, and taught by people who had never worked at acoustics, and had never made a steam test on a boiler in their lives, and never worked in a laboratory. It was futile as far as its technical applications were concerned. I had a little breeze with the Science and Art Department, and I denounced them violently for their neglect of the teaching of optics; and they agreed finally that their Honours course in Sound, Light, and Heat might be modified, and that a student might take any two of the three, but at the elementary stage he must take all three subjects, or none at all. The numbers have dropped and dropped in those classes. They used to have 1300 people coming up for examination, but I should be surprised if it is anything like one-third of that number at the present time. They have killed the teaching by putting it on wrong lines. They do not have payment by result as they used to have; the payment now certainly ought to encourage local effort in the different localities to create classes on somewhat different lines. That is speaking of the lower

kind of education.   Is the education given at the universities?   Again, I cannot help blaming the universities for the state into which optical teaching has drifted.   Not one word, of course, shall fall from me against the successors in the chair of Sir Isaac Newton at Cambridge; to Sir George Stokes we looked up as our Nestor in everything pertaining to optics.   To Professor Larmor, his successor, England owes an enormous debt for the genius he has brought to bear on many problems of mathematical physics, and physics in general.   But the teaching of the colleges and the university teaching at Cambridge—well, what is it in optics?   They call it optics, but it is really purely mathematical gymnastics applied to the optical problems of a hundred years ago.   I do not think there is really what one can truly call optical work going on at Cambridge.   The developments that have gone on there in the Cavendish Laboratory particularly, splendid as they may be in physical chemistry, electro-chemistry, and other branches, have not taken hold of what is wanted for the pioneer of optical research in this country.   The fact is, you cannot expect in these days a professor of genius, whose whole soul goes out in electro-chemical work, to devote himself to research in optics.   It needs an optical professor to investigate and make researches.

The same kind of criticism applies to other universities.   I do not believe that in the whole of the universities in this country optics is being taught by any man who ever had any real optical training. It is being taught by men who have learnt optics from the mathematical end, and have not obtained their mathematics for optical purposes.   We come down to the university colleges and the polytechnics, which are spread so widely over the country.   I am afraid, so far as the London polytechnics are concerned, outside this particular Institute where we are assembled, the subject of optical teaching is in a doubtful position, merely taken up as a small branch of physics.   Two things interfere very largely with any hope of progress—namely, the worship of numbers.   A thousand students learning typewriting is thought a more important thing than a dozen earnest fellows putting their backs into optics.   There are classes in photography largely frequented by people who want to touch up a negative, and not by people who desire to learn anything scientific about photography.   Optical teaching, I am sorry to say, is very largely at its lowest conceivable ebb.

A reference has been made to the action of the Spectacle Makers' Company.   I have myself the honour of being a citizen and a spectacle maker, and, although I have had no part in the inception of that scheme, I have from the very beginning acted as an examiner.   The criticisms which Dr Walmsley has passed are certainly not without some ground; but I beg you to remember that the Spectacle Makers' Company, although an old-established Guild dating back to Charles II., is not one of those twelve wealthy Guilds with large funds at its disposal.   It is one of the minor Guilds, one of those which has practically no funds at all of a corporate character available for this purpose, and it has been bound, therefore, to consider what it could do without the expenditure of funds which it has not got. Its scheme, with all the criticisms that may be passed upon it, has certainly done something to evoke in the opticians of this country an ambition which they did not have before of doing something or other to make themselves a little better qualified for the title of " optician."   It is quite true the criticism is perfectly just that the scheme of certification of the Spectacle Makers' Company has not succeeded to the extent it ought to have in creating in the younger opticians all over the country a desire to gain a complete and thorough training.   Those young opticians have been much too anxious to go by the shortest cut, possibly to the winning of a certificate by merely passing an examination; and if cram has been too much in evidence, they have at least done something towards learning that there is a wider world of optics they have not yet entered.   In my duty as an examiner I have had to interview, criticise, and examine *viva voce* every one of the men who has presented himself for the higher certificate.   An examiner face to face with a candidate is able to chat and talk with him, and I have availed myself —I hope not unjustly—of the opportunity to elicit a little information, which I should not think of counting either plus or minus in the examination, but information as to what the candidates were doing.   My heart has been torn sometimes to see these young fellows coming up and hoping to pass an examination for which they were hopelessly disqualified, because they had never had any training

at all, and had never been told apparently what training they might have had; they had never been taught to realize their opportunities. I have had a candidate, for example, who had been living and working for seven years not 100 miles from Ludgate Circus—I will not be more precise—who had been preparing for a year and a half to go up for the examination of the Spectacle Makers' Company in optics. I asked him which classes he had been attending, and discovered that he had not attended any, but had had a few lessons privately. He said there was no class near him. I asked him if he had not heard of the Birkbeck Institute, that had a well-equipped laboratory? No; he had not heard of it, or thought it worth while to enquire. He had never gone to the City of London College at Moorfields; it had never occurred to him to ask whether the Northampton Institute could tell him anything. These Institutes exist, and were not made use of by the very candidates who ought to be preparing. It was a great exception to find any candidate who had ever been inside a physical laboratory, much less a purely optical laboratory. I have had candidates also from the country, and the same thing occurred. I had a candidate from Glasgow, and it had never occurred to him that there was a West of Scotland Technical College, or even classes at the university that would help him. He had simply taken private lessons for a fortnight, and came up to London to the examination. I asked a candidate from Bristol if he had been to the Colleges there—to the University College or the Merchant Venturers' College? No, he had not; he had simply studied it from books. That is the state in which the candidates come; and, if it is the kind of preparation they think necessary for learning optics, it is a very bad state of things. Ought not the opticians of England to say to these candidates that cramming is the thing they must avoid; that they must go and learn something in the laboratories, in the physical laboratories of the colleges and schools up and down the country wherever they reside? Make that a part of the whole thing that they should bring a certificate that they have attended and made the experiments. It would help them towards their examinations. Let them say, "Let us have the certificate that you have really had a training somewhere before you come up to be examined." I hope we shall induce the Spectacle Makers' Company hereafter to require some such thing; but do not blame the Spectacle Makers' Company for not having required it. Remember the position they are in. They are in the same position as the Pharmaceutical Society was forty years ago when the certification of pharmacists first came up. There were men practising pharmacy who had never had any training, but they were compelled to recognise them as qualified pharmacists also. It was only when the next generation grew up that they could insist on really proper training, with examinations to satisfy the requirements of the Pharmacy Act. The Spectacle Makers' Company is in that position to-day; it is practically compelled to make its examinations easy to the men who are in the trade, in order that the trade shall not be left outside; and the younger men who come in hereafter to the examinations may come in only when they can show a certain amount of qualification.

Now it is quite deplorable to find the kind of information these candidates who come up have in optics. I have candidates, for example, who do not know in the least how to read off the graduations of a circle, who do not know, indeed, what the word vernier means. I have candidates who cannot tell me the difference between a spectroscope and a stereoscope. I ask them what astigmatism is, and they say it is a difference in the refraction of the meridian of the eye. When I ask them what a meridian is they scratch their heads and cannot tell me. I ask what refraction means and they tell me "something or other about glass and the direction of the light." I ask them the refractive index, and they say it is a relation of the sines of the angles of incidence and refraction. I ask what is a sine, and they say they do not know. It is one of the preliminary things of an ordinary scientific education of the simplest kind, but they have never heard of it. They have been pursuing optics perhaps eight or ten years in the shops, and have never mastered those simple things until they have crammed for examination, and they hope to become qualified in that way!

Now let me turn to the question of the creation of a real Optical Institute of which the rudiments exist, thanks to the energy of Dr Walmsley and his colleagues, in this place. The Technical Education

Board, which ceased to exist a year and a half ago, was, I know, very favourably disposed towards this. A deputation headed by Mr Beck made a real impression, and I thought everything was in favourable train and we should have had the foundations laid long ago for a new institute. Unfortunately, the curse of this country, politics, intervened; politics upset the Technical Education Board and threw the whole of technical education work, together with primary education, upon the County Council, and the officials and members of the County Council's committees are absolutely overwhelmed with questions of drainage and ventilation, and non-provided schools and conscience clauses, and goodness knows what. They are so swamped that they cannot look at their own scheme; they cannot let the thing go on on the lines it was going on because they are overwhelmed with other work. This ought not to be. Good work in this most important and most neglected of branches of science ought not to be stopped because of the plethora of work at the bottom end of the scale. I hope some kind influences will be brought to bear on the County Council to insist that this shall not be hung up for another year. It ought to be proceeded with, and proceeded with on the lines previously accepted, and which had been set forth by Dr Walmsley. Until we have a really scientific and technical optical school, a monotechnic devoted to optics, we shall never have optical teachers properly trained, and we shall not have an ideal set before the teachers of optics in this country—we shall not have the short summer courses of laboratory work which will be most important in the future. The whole progress of technical optics in this country is being retarded by the circumstance that this central institute is at the present moment hung up between heaven and earth, and no one exactly knows when it will begin or even whether it will begin. It is the most important thing we ought to go for, the point straight in view we are all certain about. It is the obviously right step to promote the optical industries of this country. Why do not we insist that it shall be done; and when hundreds of thousands of pounds are being squandered on all sorts of technical education of a miscellaneous kind, a small fraction of that should be devoted to something which is clear, definite, and useful, and which we are all agreed is the right thing to promote. (Cheers.)

Mr C. Beck.—When I had the honour to represent the trade before the Technical Education Board there were three or four points which I endeavoured to make clear. In the first place, it was considered—and the questions that were asked of the deputation after the original address had been presented showed—that the idea still existed with members of the Board that the optical industry was a very small matter, and was not of much consequence anyway. There is no doubt that a large number of people have been under the idea that the optical industry in any country was only a very small matter. There can be no question that this was an entirely erroneous idea; that the optical industry was not a small one, although of course it is only a small branch compared with such an industry as engineering. I produced figures at the time which were of considerable interest, as they showed the amount of turnover in optical instruments, amounting to a very large sum. I do not think there is any further need to enlarge on that when you can see the exhibition downstairs. At the same time it is perfectly capable of proof and cannot be gainsaid that when taken as a whole it is a very large industry indeed. The next point I was particularly anxious to bring out, which I need say very little upon—was that optical education as considered from the optician's point of view was extraordinarily bad. We have heard so much on that, however, that I need not further refer to the absolute lack of any decent text-book in this country that will give assistance to the optician, to the lack, except in one or two efforts recently made, of any good teaching in this country, and the fact that any optician who wishes to study his science must go to Germany in order to do it; he must read German text-books or be taught in Germany. In mixing among scientific men in this country as a member of the trade, I constantly have the finger of scorn pointed at us as a trade, and am constantly told of the magnificent concerns that are in existence in Germany, and the magnificent scientific work that is being done by the optical institutions there, and contrasting with all that the position of the optical industry in this country. But, gentlemen, I have never had the finger of scorn pointed against the English professors,

I think it would be quite as fair that the finger of scorn should be pointed against the professors in this country, with the exception of a few brilliant men, compared with those in Germany. If you look through some of the German catalogues of optical instruments you will find that some of those catalogues are actually written by professors of universities, and there is a relation between the practical and scientific men which does not exist in this country. Possibly it may be due to some extent to the opticians themselves, but I can scarcely believe the fault is entirely on our own side. Neither do I think it is on the side of the professors. I think it is the examination system which is going far to ruin our education in this country. We have heard to-day that many of our mathematicians do not deal with practical work. I have no doubt there is a good deal in it, but possibly not so much as is generally supposed. For instance, if a lens is to be calculated which shall be free from aberrations, is it taken to the Professor of a University to calculate it, or to an optician? I think in this country the lens is taken straight to the optician, and whether he does it by calculation or by rule of thumb, he is the only man who can do it. I do not for a moment wish to refer to such brilliant work as the Physical Optics at Cambridge and other parts of the country, but taking the question of geometrical optics solely, I think you will find that until the last year or two even the despised opticians knew a good deal more about it than most of the Professors. In a lecture that Professor Thompson delivered on this subject some years ago, there was one expression he made use of, which was extremely amusing, and hits off in a very terse manner the resentment that is felt by some of us at the expenditure of public money. He stated that in most of the polytechnics there was a great deal too much "polly" and too little technics. It is exasperating to men who are struggling to get good education, of use in the practical conduct of their business, to see huge sums of money squandered for wood-carving and fancy needlework, I think it is scarcely necessary to point out that the whole trade is extremely anxious for something to be done. When I had the privilege of leading the deputation I had the honour of passing in a long list of names of those who had given their adhesion, including nearly all the firms whose exhibits you will see downstairs, and there cannot be any doubt that the optical trade is not only in favour of such education, but for two or three years, both through the efforts of the Spectacle Makers' Company and the Optical Societies, and various meetings, has been clamouring for it. I think one of the main objects of this Convention should be to bring before the public the importance of technical education, and to insist that no stone should be left unturned that might lead to the establishment of such an Optical Institute as Dr Walmsley has so fully sketched out.

Mr COUNCILLOR BLACK (Brighton).—As a member of one of the provincial Education Committees, that of Brighton, I should like to say how little attention is really devoted by provincial Committees to the subject of optics, and I cannot help thinking that this is very largely due to the want of realisation, which Mr Beck has pointed out, of the many ramifications, the many trades, that are affected by optical technics. The issue of a circular to provincial Education Committees, pointing out the great need there is in well known-branches of technics for optical education, might have the result in many cases of causing more attention to be paid to the teaching of optics in the technical institutes throughout the country. It has been realised, I think, by Education Committees, that the subject of chemistry concerns a very great number of branches of industry. Perhaps this was fostered by Lord Beaconsfield's dictum that the manufacture of chemicals was a good index to the prosperity of the trade of the country. As a consequence it has been perhaps realised that the study of chemistry is essential in very many branches of industry. But it has not been realised in how many directions an accurate knowledge of the laws of light are involved, and I cannot help thinking that a circular issued to the Education Committees throughout the country, calling their attention to this particular fact, would be effective in causing the instructors in physics to be reminded of their duty to pay more attention to the subject, and it might possibly lead to the establishment of classes for the training of opticians. If a visit could be arranged

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to provincial centres by some capable lecturer who could point out more fully the line of thought that I have suggested, in the same way that we have capable engineers coming to tell us how engineering enters into so many branches of activity, and art directors coming to tell how all children should have an elementary training in art, I cannot help but think very great impetus would be given to optical education throughout the provinces.

Mr CHALMERS.—Dr Thompson has told you of the worst side of optical knowledge in the optical trade. My impression of the optical trade, coming to it as I did some years ago quite as an outsider, was how much the practical manufacturing optician really knew about the subject. Many of the "rule of thumb" methods which Mr Beck has spoken of are true scientific solutions of the problems involved. The men who use the method perhaps do not know quite enough about it to express the method in scientific language, and it is one of the things we hope for our institute that we shall be able to take these empirical methods that are known, and express them in scientific language, and develop the principles underlying them, and apply them in other directions to other problems. We hope also to be able to take the solutions which individual workers have found, and apply them to other problems of the trade, and in return bring back solutions that have been found elsewhere. We know the busy men, the men who have to solve important problems every day in their optical work, are much too busy to publish their results and are too busy to carry their results beyond the stage of satisfying the practical problem. It is our duty to develop those solutions and carry them much further into the field of practical optics. I have noticed in the work we have done in technical optics, especially in teaching optics, that there is no one subject of optics which is not very materially assisted by a knowledge of some other, probably the most remote branch of the subject. I have an illustration which may be of some interest to you. I had a problem to solve with regard to the testing angles of certain prisms. The solution of that might not have been a matter of very practical importance, but the result of that solution was to make a very considerable improvement in the prism which was used for lighthouse purposes. I think our institute might very well devote a portion of its activity to the development of principles found to be successful in one branch of optics, and their application to other branches.

At the conclusion of the discussion, notice of the following resolution, in the name of Mr CONRAD BECK, was given :—" That the Optical Convention hereby expresses its cordial approval of the prospect for founding an Optical Technical Institute for the training of opticians in the scientific principles of optics and their technical applications, which it regards as a matter of industrial importance to the nation ; and in view of the backward state of optical teaching in this country, it urges the London County Council to push forward, as a matter of pressing need, the foundation of such an Institute on the lines of the scheme which was under the consideration of the late Technical Education Board."

This resolution was discussed and adopted at the Business Meeting (p. 242).

THE EARLY HISTORY OF TELEPHOTOGRAPHY.

By Major-General J. Waterhouse, I.A.

A FEW years ago I was led to investigate the early history of the camera obscura, and the application of what may be called the "pinhole" principle to the observation of eclipses, by passing the solar rays through a small aperture in a darkened room, long before Porta's time. In the course of these investigations I was able to bring together a great deal of interesting and practically new information regarding the early development of the camera obscura from the old problems of Aristotle relative to the rounding of the sun's image when projected on the ground through angular apertures, and his observations of eclipses on the same principle. I found also that J. B. Porta had little claim to the invention of the camera obscura, as Libri had already shown that he had been anticipated in his description of the simple camera obscura without a lens by Leonardo da Vinci and Cæsariano, while it had actually been applied to solar observations by Maurolycus, Reinhold, and Gemma Frisius long before his time. The first suggestion of using a convex lens to collect and project the images of outside objects on to a screen in a darkened room was made by Daniel Barbaro in a Treatise on Perspective, published in Venice in 1568, or about twenty-one years before it was given out by Porta in his second edition of the "Magia Naturalis," as a great secret of his own. Barbaro also suggested the use of a diaphragm for improving definition.

The first idea of a portable camera obscura seems due to Kepler and his arrangement of a little tent with the object-glass of his telescope as lens is described by Sir Henry Wotton in a letter to Lord Bacon about 1620. To Kepler also is due the suggestion of the use of combinations of one convex and one concave lens to extend the focus of the former and enable enlarged images to be projected on a paper screen—a method which was very fully developed by Fr. Scheiner, and formed the foundation of modern telephotography—the application of this principle to photography lying dormant till Porro took it up in 1847, though properly constructed telephotographic lenses were not used till 1890.

These investigations were published in three papers : "Notes on the Early History of the Camera Obscura" and "Notes on Early Teledioptric Lens-Systems and the Genesis of Telephotography," read before the Royal Photographic Society and published in vols. xxv. and xxvi. of its *Journal*, 1901 and 1902, and "Historical Notes on Early Photographic Optics," published in the *Journal* of the Camera Club, 1902.

On this occasion I have confined myself to the more optically interesting subject of teledioptric combinations leading to our modern telephotography, especially by Kepler and Fr. Scheiner, and their application of the Galilean telescope for projecting enlarged images of the sun.

I need not enter into the history of the telescope, but there are some very interesting papers in the first series of the *Phil. Mag.*, vols. xviii., xix., and xx., signed "D.," in which the author gives extracts from old English works tending to show that the telescope was known first of all in England, and drawing attention to the optical work of Roger Bacon, Robert Recorde, and the two Digges (Leonard and Thomas), and John Dees. In following this up there seemed very strong evidence that in the sixteenth century practical optical science was more advanced in England than elsewhere, but unfortunately the records are very imperfect.

There are several passages in Roger Bacon's works which show that he had a fairly good idea of the use and advantages of a telescope for magnifying the images of distant objects and bringing them nearer, also for projecting enlarged images, and it seems probable that he had constructed instruments of the kind. Recorde, in his "Pathwaie to Knowledge" (1574), mentions one he set up at Oxford, and Thomas Digges ("Stratioticos," 1579) tells us how his father, Leonard Digges, was able to construct perspective glasses from reading one of Bacon's books of experiments. The arrangement is described in

"Pantometria" (1571). By some this has been thought to be a camera, but from the description it suggests an anticipation of the reflecting telescope, by which the image formed by a concave speculum is received and magnified by a convex lens. It is not improbable, therefore, that Bacon's telescope was on a similar principle. From Digges' account of his own arrangement it may possibly have been applied to graphic delineation; he says further details had been given in another work on perspective glasses, which unfortunately has been lost. John Dees also refers to "perspective glasses" as in common use some time before the assumed date of the invention of the telescope in 1609.

Further light is thrown upon this subject in a little treatise "On the Properties and Qualities of Glasses for Optical Purposes, according to the Making, Polishing, and Grinding of Them," written by William Bourne, Portreve of Gravesend, about 1585. It is in MS., No. 121 of the Lansdowne Collection in the British Museum, and was published in J. O. Halliwell's "Rara Mathematica."

The account he gives of the combination of perspective glasses is interesting, corroborating Digges and Dees, and showing that some form of telescope was known in England before the invention of the so-called Galilean telescope in Holland. Bourne describes clearly the effect of a large shallow convex lens, a foot in diameter, and a quarter of an inch thick at the centre, in magnifying objects seen from behind it, and the reversal of the images beyond the focus, which had already been observed by Leonardo da Vinci, and says that the amplifying effect will be increased by receiving the beam that comes from a well-polished concave speculum. Want of means, however, prevented him from carrying out the construction of these glasses. He says nothing about the projection of the images on a screen. In his "Inventions or Devices" (1578), he has described arrangements of mirrors, and of a large convex lens, with a plane mirror for viewing distant objects, similar to an arrangement of Cardan's.

Maurolycus seems to have been the first to investigate the optical properties of convex and concave lenses, and the formation of reversed images at the focus of the latter ("Diaphaneon," 1534, I. Prop. 23). He does not, however, suggest any practical use of lenses in this way. Hevelius says that he discussed the theory of the telescope, but I have not been able to find the passage.

In the "Magia Naturalis" (1589) J. B. Porta mentions a combination of a concave and convex lens with which, if properly adjusted, both near and distant objects could be seen larger, but clearly. Upon this passage claims have been made for Porta as the inventor of the telescope, but in his more scientific and later treatise "De Refractione Optices parte" (1593), it is curious that nothing is said of the projection of images by a single convex lens in the camera obscura, or of any such combination of a convex with a concave to form enlarged images, either by viewing or projection. The passage in question rather seems to have been inspired by Bacon, from whose "Perspectiva" Porta quotes elsewhere.

The application of the simple camera obscura to the observation of eclipses, which started with Aristotle, was discussed by Roger Bacon, Peckham, and Vitellio, but Maurolycus seems to have been the first to have put it in actual practice, and in the third dialogue of the "Cosmographia" (1543), he describes how he ascertained in this way that the diameter of the moon was smaller than that of the sun. It was, however, in Germany that this method of observation seems to have been developed by Reinhold and his pupil Gemma Frisius, who has given a very full account of the observation in this way of the phases of an eclipse of the sun in 1544, then by Moestlin and his pupil Kepler, the latter applying it to the observation of eclipses in 1598 and 1600, and, in May 1607, to the observation of a transit of Mercury. Tycho Brahe also used the same method.

One of the first applications of the Galilean telescope was as an adjunct to the camera for astronomical purposes, and it is here that the history of telephotography begins, and Johann Kepler, the eminent astronomer, led the way. He was the first to investigate the camera obscura theoretically and practically, he completed the work of Leonardo da Vinci, Maurolycus, and Porta with regard to the theory of vision and the mechanism of the eye, and showed its exact analogy with the camera obscura; he was also the first to put the latter into a portable form for sketching purposes.

Space does not allow us to discuss fully Kepler's work in this direction, but it is to be found in two

of his works "Ad Vitellionem Paralipomena" (1604), and his "Dioptrice" (1611). In the former he discusses the old problems of the passage of light through small apertures, and the solutions of them by Vitellio, Peckham, and others, but he did not know of Maurolycus. He also refers to his masters, Reinhold, Gemma Frisius, and Mœstlin, who taught him to apply the problems practically in observing eclipses, and mentions some observations of the sun he made in this way in 1600. He also explains the use of the camera obscura in forming images of outside objects.

The "Dioptrice" deals more particularly with refraction and the use of lenses, the convex, concave, concavo-convex, plano-convex, and other forms and their combinations being described.

In Problem 43 "To depict visible objects upon a white screen with a convex lens," he discusses the use of the lens in the camera obscura and gives the theory of its action. In the next he explains the reversal of the image.

In Prop. 88 he discusses the projection of visible images erect by means of two convex lenses.

Props. 104 and 105 are, however, of more interest in connection with our subject, for in these Kepler first discusses the application of the combination of a convex and concave lens, known as the Galilean or Dutch telescope, to the projection of magnified images of visible objects on to a screen, on practically the principle of the modern telephotographic lens.

In Prop. 104 he shows that if the rays from a point, which after undergoing refraction through a convex lens would converge, are intercepted by a concave lens before they reach their point of convergence, either the distance of this point will be extended, or the rays will be carried on parallel, or lastly, they will diverge again.

The demonstration given is imperfect, but shows that he recognised the three cases.

In Prop. 105 he propounds the problem of depicting visible objects with a concave and convex lens upon paper larger than by a single convex lens but reversed.

He does not go into the question of the adjustment of the relative distances of the convex and concave lenses and the effect on the size of the projected images, but says that in instruments showing visible objects larger and distinctly, the concave lens cannot effectively be far from the point of convergence formed behind the convex lens.

It is evident from this that Kepler understood the principle of this combination as applied to enlarged graphic delineation and is entitled to the credit of its discovery, though, as we shall see, it was more fully developed by Father Scheiner.

One of the earliest applications of the telescope was to the observation of sun-spots, and, as might be expected, they were observed by several people all about the same time, between 1610 and 1611, and there has been a good deal of controversy as to priority; but according to Sir N. Lockyer, Galileo began his observations in October 1610, being followed by Father Scheiner in 1611, and by Fabricius in the same year. It was claimed for our own countryman Harriot that he observed them in December 1610, but later investigations show that it was a year later.

It was soon found that such observations were dangerous for the eyes, and in order to get over this Johann Fabricius and his father adopted the old method of darkening a room and receiving the sun's image upon a piece of paper, which they kept moving so as not to confuse the sun-spots with marks on the paper. They found the method successful, and combining it with the telescope they made a number of observations of the number and shape of the spots, their courses and periods, which were described in J. Fabricius' "De Maculis in Sole Observatis" (1611).

Another contemporary observer, Simon Marius, seems to have used the same method, as noted in his "Mundus Jovialis, Anno M.DC. IX., detectus ope perspicilli Belgici" (1614).

The first to apply the telescope itself to the graphic delineation of sun-spots, etc., was Father Christopher Scheiner, in 1612. An account of his observations and methods were published in 1630, in his "Rosa Ursina," a handsome and well-illustrated volume. The principal feature of his optical work is its thoroughly practical character and the way in which he discusses the advantages and

disadvantages of the different methods described. The well-engraved illustrations are also valuable for the light they throw on the actual methods of observation then current.

The most important chapter in the book as regards telephotographic projection is the twenty-eighth of the second book (p. 124). After discussing the difference in the refractive action of concave and convex lenses, and the effect of placing a concave in front of a convex at or beyond its focus, or immediately behind the convex, he considers the case in which the concave is placed a little in front of the focus of the convex, so that a real image is formed on the screen, larger, more distinct, and at a greater distance than if the convex were used alone, and he explains it by means of a figure which might well serve as an illustration to a modern treatise on telephotography.

He goes on to show that the position of the concave lens is not fixed, but has a considerable range between the convex lens and its focus. The nearer it is to the focus, the smaller is the image thrown on the screen, but clearer, and the screen can be at its shortest distance from the concave. Conversely, as it approaches the convex, a larger image is formed, but not so clearly, and the distance beween the concave and the screen will be increased. The picture formed by a certain adjustment of the lenses and screen will be confused if the screen is moved in either direction to or. from the concave. He notes, however, that this confusion is not immediate, and that there is a certain latitude, especially if long-focus lenses are used in a proper way. In the same way, if the screen on which the image is correctly depicted remains fixed, and the concave lens alone is moved forwards or backwards beyond the limits of sharp definition, both the lens and the screen will be put into the position of confusion. He notes the changes of colour and brilliancy of the images accompanying the movements of the lens. The same occurs if the convex lens alone is moved, or if the object approaches or recedes from the convex. A deep concave lens requires a longer distance from the convex and less from the paper, and will form a larger image than a lens of less concavity (or longer focus), which would require a shorter distance from the convex and longer from the paper, but give a smaller and more intense image. With the same concave, a convex lens of short focus requires less, and one of longer focus greater, distance.

He next considers the effect of a concave lens between two convex lenses as used in the telescope for vision, though it is also applicable to the helioscope. In this case the concave is near the back convex, and the two together act much in the same way as the single concave. When brought closer to the object-glass a larger image is formed on the screen, which has to be moved a little back and *vice versâ*.

In chapter xxxiii. he deals with the selection and arrangement of the lenses and the construction of the observing telescope, the use of diaphragms for cutting off the oblique pencils. He then gives some practical problems for finding the focus of the convex object-glass, from which the proper position of the concave lens slightly in front of it can be ascertained. It does not depend on the power of the concave lens, but on the focus of the convex, and if found for one others can be used, the screen being adjusted to suit the image. The focus of the two lenses being known, the length of the tube required can be ascertained exactly, a little excess being allowed, and if it is intended to use concaves of different powers, the length of the tubes must be arranged accordingly.

With the same lenses the size of the image can be increased by moving the concave towards the convex, and drawing away the screen from the concave as much as may be necessary.

In his "Apiaria" (1641) Marius Bettinus has discussed the projection by means of lenses, the formation of a reversed image by a single lens and its being made erect by a second lens. He says that at the best with ordinary lenses the rays do not all converge at one point, and thus produce an indistinct reversed image. By making the image erect with more lenses there is loss of illumination, and rays are cut off which would give effect to the picture. He recommends the hyperbolic lens because it focusses rays and images at one point ; it never causes confusion ; it has two foci of perfect definition ; it does not require another lens to erect the image ; whether the image produced be reversed or direct it is far more perfect than with any other lenses. He gives a figure showing the image of an outside object passing first through a small aperture on to his hyperbolic lens and giving a sharp

reversed image in front of the focus, and a second sharp but erect image beyond the focus, and says that the second image will be almost the same in every way, except its erectness, as the first.

The idea of these hyperbolic lenses seems to have originated with the view erroneously entertained by Kepler, that the form of the crystalline lens of the eye was hyperbolic. It was taken up also by Descartes.

In his "Selenographia" (1647), Hevelius has given a good deal of information regarding the properties and qualities of optical glasses and their preparation. He describes a method of testing telescopes by projecting images of the sun with them, and then comparing the size and definition of these images. He also discusses observations of sun-spots and faculæ with the helioscope constructed with coloured glasses and Scheiner's other apparatus, and recommends a modification of the latter, in which the telescope is fixed into a spherical wooden block which can be moved in any direction in a socket fixed in a window of the darkened room of the observatory. In his later work, "Machinæ Cœlestis" (1673), he describes and figures a further improvement, in which the instrument is fitted with an elaborate arrangement for properly adjusting the paper screen, and keeping the image of the sun in position following the movement of the earth. It has the same ball-and-socket adjustment for holding the telescope.

In his "Ars Magna Lucis et Umbræ" (second edition, 1671), Fr. Kircher has described a great many optical combinations; but as regards the use of the telescope for projection, he has only very briefly noted the effect of a concave placed behind a convex and a little in front of its focus, and the changes produced by moving the lenses to and fro, also the use of two convex lenses or a concave between two convexes. This is mostly taken from Scheiner. He points out the advantages of hyperbolic lenses, but also the impossibility of constructing them.

The French mathematician, Milliet Deschales, has discussed the effects of the combination of lenses, both for vision and projection, in the second volume of his "Cursus, seu Mundus Mathematicus" (1674), containing the *Optica, Perspectiva, Catoptrica*, and *Dioptrica*. In the second book of the latter, after other problems relating to combinations of convex with concave lenses, he deals in Prop. 40 with the case of a concave glass placed behind a convex lens, giving a larger and a more distant image than the convex alone, and gives a curious demonstration of it, tracing the course of the rays backwards from the projected image to the object, and showing that the rays will go back the same way from object to image. In Prop. 42 he considers the principles and construction of the Dutch or Galilean telescope, and notes the effect of stronger or weaker concaves and their position behind the convex. He also shows that for the projection of an image on paper the tube requires to be longer than for visual observation. Further details of the latter method are given in Prop. 49. A figure is given of his arrangement, which consists of a horizontal board, with a transverse upright at one end holding the paper, and two others at the other end to hold the telescope to it. A wire was placed between the concave and the paper to mark the vertical circle by its shadow, and the position of the spots relatively to it, the solar image being reversed. He then discusses the use of the telescope with two convex lenses for the same purpose, the eye lens being beyond the focus of the object-glass, so as to throw a distinct image on the screen. He does not seem to have made any marked advance over Scheiner.

In 1675 Fr. Zacharias Traber published a treatise on optics, "Nervus Opticus," which in some respects seems an advance on its predecessors, being clear and practical, and he introduces calculations for ascertaining the focus of a lens from the size of the image it gives of an object, and the converse for the distance or size of the object. In Book III. chap. xix. Prop. 4, he deals with the projection of erect images on a screen in a dark room through a system of two convex lenses, and in chap. xxi. Prop. 2, he considers the combinations of concave and convex lenses, and the calculation of their distances, a problem which Scheiner did not attempt, but preferred to work out practically. In Prop. 10 he discusses the projection of images with a combination of a concave with a convex lens. His demonstration is something like Kepler's, in attributing the enlargement of the image to the mere divergence of the rays after passing through the concave.

The first edition of Johann Zahn's "Oculus Artificialis Teledioptricus" was published in 1686, and the second improved one in 1702, and in this is for, I think, the first time, figured a small camera fitted with a compound lens, convex with concave, for throwing an enlarged image upon the focussing screen, which in its proportions and application comes very near our modern telephotographic objectives.

In chapter iii. of the second part of his book, Prop. 10, he demonstrates the old theorem of the formation of a larger and more distinct image, by placing a concave lens behind the convex and a little in front of its focus, giving the same figure and demonstration as Deschales. Then follow four corollaries showing (1) that a concave lens in this position can on transmitting the image portray it larger than any single convex lens can do at the same distance; (2) that the artifices used in show-boxes and camera obscuras to greatly magnify the images transmitted at a short distance may be inferred; (3) that a large image may thus be produced with such a combination at a short distance instead of using a single convex lens of greater focal length, and so a more contracted and shorter tube may have the same or even greater power than a longer tube with an ordinary object-lens; (4) that as the concave lens recedes from the convex a smaller image is formed, and on the contrary when nearer the convex a larger image will be formed at a greater distance. Zahn has thus very clearly recognised the advantage of the combination in practically shortening the focal length and distance of the screen for the size of image obtained.

He has very fully discussed the Galilean telescope and its use for astronomical observations as proposed by Scheiner. He has also described several practical applications of the teledioptric principle, and at p. 689 there is a description with figures of what he calls a parastatic box for showing enlarged images, in which a convex lens is combined with a concave, very much in the same way as in our modern telephoto lenses—the image being received on a mirror and reflected upwards. His convex lens would have a focus of $\frac{60}{100}$ of a foot, say $7\frac{1}{4}$ inches; a plano-concave lens of $\frac{25}{100}$ focus, say 3 inches, was fitted in a smaller tube sliding into the other, and the focus adjusted by the rules laid down previously. The two tubes were fitted into a larger one and fixed on the camera. Ordinarily, the images were reversed, but could be made erect by means of the mirror, as shown in the figure. It will be noted that the proportions of his compound lens are very close to modern instruments.

If Zahn made little advance in the theory of the tele-objective combination, he seems to have done so in practice, for I know of no other writer before him who has figured portable instruments of this kind. His book is a valuable repertory of information on early optics, on account of the numerous extracts from and references to earlier writers, besides much original matter of his own.

William Molyneux of Dublin seems to have been one of the first to deal with the various problems connected with the use of the telescope for projection by calculation mathematically, in his "Nova Dioptrica, or Treatise of Dioptrics" (1692). He gives the solutions of a great many optical problems connected with single lenses, and in Prop. 17 (p. 74) enunciates the problem of the compound tele-objective. "A convex glass being given, with a concave of larger sphere, the concave being placed behind the convex, at any distance less than the focal length of the convex, 'tis required to find the place of the compound focus, or distinct base of the two glasses." And after demonstration gives the following rule:

From the focal length of the convex subtract the glasses' distance. Mark the difference, then say: As the focal length of the concave – this difference is to the focal length of the concave, so this difference is to the distance of the distinct base from the concave. He gives practical examples.

In Prop. 18 he gives the solution of a similar problem, in which the concave is in front of the convex and towards the object, and at p. 77 there are solutions of these two problems by J. Flamsteed, then Astronomer-Royal, which are far more elaborate than Molyneux's, but give practically the same result. In his figure Flamsteed shows a plano-convex and a plano-concave arranged with their plane sides outwards, as Scheiner has done in his figure.

Molyneux has also given a problem for determining the breadth of the distinct base, with combinations of two convex lenses, or a concave and a convex, either being in front, the breadth of the object

being given, or the angle it subtends before the outermost glass, and if it be a near object its distance from that glass.

Sir John Herschel in the eighth edition of the " Encyclopædia Britannica," article *Telescope*, has drawn attention to Prop. 33, chapter vi., of Christian Wolf's "Elementa Dioptrica," showing how the astronomical telescope may be shortened from its ordinary length, but at the same time give a larger image by the insertion of a double concave lens, so that the focus of the object-glass should be behind it and nearer its virtual focus. He refers the principle of the Barlow telescope back to it, saying that the interposition of an uncorrected concave with an achromatic o.g. would destroy the achromaticity of the image, but that a concave lens could be made achromatic as well as a convex. Wolf, however, was not the first to discuss this principle, and Herschel's remarks would apply equally well to Kepler's or Scheiner's arrangements, but are interesting because they form the connecting-link between the old unachromatic tele-dioptric combinations and the later achromatic ones.

Dr Robert Blair and Professor Peter Barlow both endeavoured to effect this object by means of an achromatic concave fluid lens, but in 1834 Dollond succeeded in making an achromatic negative lens of glasses on a principle suggested by Barlow which was found to work satisfactorily.

This improvement does not, however, seem to have been used or suggested for purposes of projection, and the method of utilising the Galilean telescope in this way does not seem to have been revived until Ignazio Porro made a combination of the kind in 1847, which was used for observing an eclipse in 1851. The positive lens seems to have been an ordinary achromatic single landscape lens, but although Porro published the principles on which his negative lens was based in the early volumes of the French Photographic Society's *Bulletin*, he gave no details of its construction. He afterwards brought out a lens called "anallatic." In 1857 the first photo-heliograph, designed by Dr de la Rue for the Kew Observatory, and constructed by Messrs Dallmeyer, consisted, like its many successors constructed for other observatories, of two converging or positive lenses, the hinder one picking up the image from the focus of the object-glass and throwing an enlarged image on to the photographic plate.

In 1869, Messrs Borie & Tournemire brought out a portable combination on the same principle for taking enlarged pictures of architectural details and other purposes of telephotography, but owing to its small intensity, and the want of sensitiveness in the plates of the time, it did not come into general use ; the same applies to Jarret's lens.

In 1870, the late Mr Traill Taylor published in the *British Journal of Photography* a simple way of obtaining a sharp telescopic view of the sun or other distant object, by magnifying the image formed by an ordinary photographic lens with a similar lens of short focus placed at the requisite distance in front of the aerial image. In 1874, acting on this hint, I adapted an equatorial camera on Mr Brothers' system, which had been used for observing the eclipse of December 1871 in India, with an eye-piece for projecting an enlarged image on a photographic plate for the observation of the Transit of Venus in Calcutta. It answered well, giving clear images magnified about seven times, the principal objective being a Dallmeyer R.R. of 30-in. focus.

Dr Adolf Steinheil, of Munich, seems to have been the first to construct a telephotographic lens on the modern pattern in 1889 for the Royal Observatory at Brussels, and in 1890 another for the Marine Department in Berlin. In 1891, curiously enough, three such combinations were worked out quite independently, and patented by T. R. Dallmeyer, in London ; Dr A. Miethe in Berlin ; and A. Duboscq in Paris ; since then they have multiplied and greatly improved.

This brief sketch of the development of the telephotographic lens from Kepler's and Scheiner's early astronomical observations is of interest, because it shows that there is still much to be learnt from good work done and recorded in years long gone by. With the improvement of the telescope for visual observation, methods of projection seem to have fallen into disuse, and when photography came into use they had been forgotten, or were found too slow for the less sensitive collodion plates formerly in use, but with the present highly sensitive gelatine plates, these old combinations with modern improvements may yet do valuable work in astronomical research, which was their original object, as well as in many other scientific applications.

STEREOSCOPIC VISION.

By C. W. S. Crawley.

THE human eyes, faulty as they may be in many ways, are in one respect an instrument of extreme precision.

As a means of instantly detecting minute differences of angle, they have a delicacy which is but seldom appreciated. Helmholtz has investigated the matter, and in his *Physiological Optics* describes experiments which show that the eyes are capable of detecting a difference between two angles of as little as one minute. This power of discrimination it is that enables us to judge distance by stereoscopic vision, not indeed to judge distance absolutely, but to say which of two objects is the nearer.

To take a concrete example. Let us place two rods or other suitable objects, the one at the distance of 2 metres and the other 2 centimetres further. Let them be nearly in the same line of sight, be perfectly evenly lighted, and have all surroundings cut off, so that there is no means of judging which is the further except the opinion that we form by looking at them. If we do so with one eye only, we can not say which is the nearer, nor could we even if they were much further apart.

With two eyes, however, no one with ordinary vision would have any doubt. One rod "looks" nearer than the other, though why it does we do not and cannot consciously realise.

This power of judging distance is common to all; it has always been advantageous to every member of the human race to judge distance, and to do so continually the whole time that he is awake. Evolution has consequently had its fullest opportunities, and has seized them with marvellous results.

How are we able to judge that one of these rods is nearer than the other? Simply by the fact that when we look at it we have to converge our eyes a little more than when we look at the other.

The proximate physical fact that we unconsciously judge by is an appreciation of the comparative muscular efforts to produce the convergence of the eyes on objects at different distances. Taking the eye-distance as 65 mm., which is about the normal, the angle eye—rod—eye at 2 m. is roughly $1° 48'$, while at 2 m. 20 cm. it is roughly $1° 47'$. The difference of convergence of the eyes on the two rods is consequently only about 1 minute, and probably 99 per cent. of the population would have no hesitation about which is the nearer, and the remaining 1 per cent. would be found not to have stereoscopic vision at all. But with all reverence for a great name, Von Helmholtz put the limit far too high. From numerous tests in all sorts of conditions and ages of men and some women, as will be seen in Table I., only two cases have been found where the angle was even half a minute. The general angle appears to be about $10''$, and really good men can appreciate with certainty a difference of 2 to 3 seconds.

In repeating the experiments there are one or two points that must be attended to, to ensure that the distance is judged only by stereoscopic means; one of the objects should be fixed, and the other close to it on a slider, so that it can be moved to or from the observer in a straight line.

The lighting must be even and regular, and should be exactly behind the observer, otherwise shadows may be of great help, and far too good results unintentionally obtained.

The background should be fairly uniform and preferably at least as far off again as the rods.

The holders, slides, and all surroundings must be cut off by a screen, so that only the rods can be seen.

Ordinary wax matches make excellent rods.

The eyes should be on a level with the tops of the matches, which should differ in height by a few mm.

A great deal of most excellent work has been done on stereoscopic vision by Dr Pulfrich of Jena, and will be found in his various papers. Among other uses, he has applied it to surveying work, two photographs of the same landscapes being taken, at a considerable distance apart, to increase the stereoscopic effect. (The Astronomer-Royal at the Cape has also worked on this line.)

The Zeiss Stereoscopic Range-Finder also is principally due to him.

One of the most beautiful uses made of Stereoscopic Vision is in astronomical work, and is described in a paper by Dr Pulfrich, read before the Astronomical Convention at Göttingen in 1902.

Photographs of the same region of the sky are taken at a suitable interval, which may be years apart, and viewed stereoscopically. Any stars that have moved during the interval parallel to the line joining the eyes stand out in front of the plane of the others, or retire behind that plane, and are spotted instantly and with certainty. This is the very finest stereoscopic effect that we can ever hope to get. Suppose we take two photos at a year's interval in a direction at right angles to the line of the sun's motion in space, that motion being about 350,000,000 miles per year, when we put the two in a stereoscope we get the effect of a stereoscopic base of about 350,000,000 miles, and as photos are now taken continually, we shall steadily enlarge that base year by year, and century by century.

Dr M'Kenzie Davidson has used stereoscopic vision for X-ray work. An X-ray photo of a leg, for example, is taken; the tube shifted a few centimetres to one side, and another photo taken. When the two photos are viewed in a stereoscope, instead of having a mere flat diagram, all the bones stand out in perspective, and any foreign bodies such as bullets, needles, etc., can be exactly localised. He has gone further. Two tubes, a little distance apart, are worked alternately. A vibrating shutter in front of the right eye is open when the left tube is one and *vice versâ*. A perfect stereoscopic effect is thus obtained on an ordinary phosphorescent screen. Incidentally the two tubes need not be equal; one may be so bad as to give an extremely weak image, and yet the stereoscopic effect will be perfect.

In the same way a man may have one eye very defective indeed, and yet have good stereoscopic vision.

It was mentioned above that an exceptionally good man will appreciate as little as 2 to 3 secs. Of course, when working with rods as described there is a feeling that the readings may have been assisted by shadows, or in some way unconsciously "cooked." The figure is, however, confirmed by tests with the Forbes' Range-Finder. In this case no such chance of error can arise, as there is no question of judging by anything but stereoscopic vision, pure and simple.

The instrument is fairly well known, but a few words of description may be given.

The object is viewed through a prismatic binocular. In the focal plane of the object-glasses of the two sides are two absolutely similar photographs of a balloon on clear glass. The two appear as one by stereoscopic vision. If they are both at the centre of the field they appear at the same distance as an object at practically infinite distance, say, the moon. One of them can be brought towards the other by a micrometer screw, and the eyes must converge a little to still see them as one object. The balloon then appears at the same distance as an object on the landscape, on which the eyes have to converge at the same angle. By turning the screw the balloon can be made to appear to approach or recede till it appears the same distance as any particular object; the range of the latter is then shown on a suitably divided screw-head. With the ordinary binocular of eight magnifying power, and about 3-in. eye-distance, or rather object-glass distance, the range that can be taken to one or two per cent. to about 120 yds., but by adding an arrangement of prisms on a base 6 ft. long, the same effect is produced as if the eyes were 6 ft. apart, and distances twenty-four times greater—say 3000 yds.—can be taken.

The pointer on the scale can be set "autonomously," if one may be allowed the expression, *i.e.* without reference to any known distance. A reading is taken of any object at suitable but unknown distance, first with the binoculars alone, then with the base added. From these two readings an accurate setting of the pointer can be made.

Very numerous trials have been made with this instrument, both in the Army and Navy and with civilians. The sailor might have been expected to have come out far better than the soldier, owing to his having to exercise his sight more, but there is no marked difference. The common private in the Army can almost always after half an hour's instruction—often indeed after only five minutes'—take a range of about 2000 yds. accurate to 20 yds. This with a 6 ft. base and eight magnification power is equivalent to 17 secs., and in a very few hours he will be taking 3000 yds. accurate to about 30 yds.

As might be expected, some men are better than others. One or two skilled Army sergeants have given very good results, but this may in some cases be due to other causes—general smartness—rather than really better stereoscopic power. But there is one observer we have come across who has distinctly greater stereoscopic power than the average. The first time he saw the instrument was at Bisley. He then looked through it at a target distant some 1140 yds. and ranged it within 2 yds. first shot. There are two of his readings that may be mentioned. A tower of Holloway College at Egham was just visible among the trees on the sky line. It was a nasty object—that is, its surroundings made the stereoscopic effect difficult to get. Five readings of this were taken, and the middle one of them—which has always been found far more reliable than the mean—gave the distance, 10,740 yds. as measured in the Ordnance Map, correct to 100 yds in 10,000. This gives 7 secs. on a bad object at very long range indeed. Another case was at Gibraltar—same observer. His readings, on the corner of a castle, were 2147, 2149, 2149, 2147, 2147, 2145 yds.

Here then we have five successive readings, the maximum difference being 4 yds., which, with eight magnifying power at 2000 yds., means an angle of 2·8 secs. as *maximum* error. His note on this says:— "The scale of instrument at this range does not permit of accurate reading to less than 5 yds., but knowing that the greatest possible precision was wanted I estimated the values of very slight differences in a set of almost identical readings. I might have entered them all as identical except the last."

When he had finished he was told by the R. E. Captain before whom he was working that the distance was 2451 yds. He thereupon checked his Range-Finder autonomously, found it perfect, and asked to have the R. E. measurements checked, as they were certainly wrong. They were re-checked, and the actual distance was found to be 2145·7 yds. (another object had been measured the first time).

Light, atmosphere, and object were no doubt perfect; but that merely means that there were no opposing elements, not that there was any assistance from any other source. The distance was measured repeatedly and accurately by the stereoscopic power, and that alone; that reduces Helmholtz's 1 minute to well under 3 seconds.

TABLE I.

Tests at 7 ft. and 23 ft. 6 in. by two observers. Wax matches with red heads level with each other and with the eye. One match can be pulled towards or away on a slide, but nothing but the matches is visible. + means the movable match was set too far and *vice versâ;* "o" means less than ½ second.

| Distance 7 ft. | | | Distance 23 ft. 6 in. | |
F.	C.		F.	C.
− 5″	− 10″		− 3·36	+ 3·36
o	+ 2·5		− 1·20	·28
− 2·5	+ 2·5		+ 2·24	+ 6·72
+ ·25	+ 1·25		+ ·84	+ ·28
− 5·	+ 3·75		o·	+ 3·36
+ ·5	+ 10		+ ·56	+ 3·92
+ 5·	o		− 1·20	+ 16·80
+ 2·5	o		·84	+ 7·84
o·	+ 7·5		+ 2·80	
− 5·	o		+ 3·92	
Mean 2·6″	3·7″		Mean 1·7″	4·2″
Algeb. Mean − ·9″	+ 2·7″		Algeb. Mean − ·37″	4·2″

TABLE II.

TEST ON VARIOUS PEOPLE AT 7 FT.

		Age.	Mean.	Max.	
1	D.H.	36	2·3″	3·6″	Engineer. Well-known tennis player.
2	E.C.	30	2·7	5·4	Solicitor. Good billiard player.
3	B.	...	4·5	9	Engineer. Good billiard player.
4	A.T.	46	5·1	9	Skilled Observer.
5	T.B.	9	5·2	9	Board-school boy.
6	M.	25	5·8	9	Physical Laboratory assistant.
7	M.H.	35	5·8	9	Lady. Very good tennis player.
8	J.R.	47	7·2	14	Physical laboratory assistant.
9	E.	46	9	11	Lady. Good tennis player.
10	C.	45	9·1	13	Skilled observer, after some practice with Forbes R.F.
11	S.H.	10	10	22	Board-school boy.
12	R.R.	45	11	36	Engineer.
13	Mi.	20	11	18	Physical laboratory assistant.
14	H.O.	5	11	45	Board-school boy.
15	g.	9	11·6	27	Board-school girl.
16	O.Ch.	70	15	22	Engineer. Skilled observer.
17	T.R.	6	15·8	31	Board-school boy.
18	Res.	50	19	31	Chemist.
19	C.P.	4	22	36	Board-school boy.
20	Hadl.	35	40	68	Engineer.

Dr R. M. WALMSLEY.—I should like to call attention to the extreme accuracy expressed by one particular observer in using the Forbes Range-Finder. In examining the results with a view to determining the degree of accuracy obtainable, we must look at the percentage error. For this particular man it is 2 yds. in 2000 in round numbers, which is one-tenth of 1 per cent.—an extremely fine reading indeed.

The Section then adjourned.

FRIDAY, JUNE 3rd.

SECTION II.

Sir W. ABNEY in the Chair.

SMALL TELESCOPES AND BINOCULARS.

By C. V. Drysdale, D.Sc.

AMONG the various optical instruments employed for the ocular examination of objects, microscopes and large telescopes have received a large amount of consideration, but the study of the smaller hand telescopes of magnifications of from 2 to 12 or more diameters appears to have received comparatively little attention. As these instruments play a somewhat important part, and are manufactured in large quantities, a short consideration of them may be found useful, especially as some erroneous ideas concerning them appear to be prevalent, and some of the principles involved in their construction are peculiar to them.

With large telescopes we are not, as a rule, limited as to the external size of our telescope, but may adopt any dimensions which will best suit the optical requirements. Again, the size of the field of view in large telescopes, except when they are used for photography, is of comparatively little importance, and we are not limited to erect images. In field-glasses portability and amount of field of view are of as great importance as perfection of image, and erect images are essential; and these limitations make the problem of satisfactory field-glass design of the same order of difficulty as the design of photographic or microscopic objectives.

We may commence by briefly enumerating the necessary and desirable features of a portable glass. They may be classified as follows :—

Essential
- Emergent light parallel for normal eye or within range of accommodation.
- Erect image.
- Focussing adjustment.
- Good central definition.
- Suitable magnification.

Desirable
- Greatest possible illumination.
- Largest possible field of view.
- Small compass and weight.
- Freedom from oblique aberrations.
- Absence of internal reflection or flare.
- Freedom from deterioration with heat, damp, etc.
- Rigidity.

In addition for Binocular glasses we require—

Essential
- Parallelism of axes.
- Equality of magnification.
- Correct or adjustable interaxial distance.

Desirable
- Maximum stereoscopic effect.
- Combined focussing motion.
- Separate focussing motion for anisometropic cases.

Part I.

Before considering any of the various forms of glasses actually in use, it may not be out of place to say a few words as to the functions and properties of telescopes in general. There are two ways in which such instruments may be regarded. In the first they are considered as instruments complete in themselves, and the function of any telescope is then to deliver a parallel beam of light and to effect an increase of the "visual angle" between the extremities of any object viewed. But we may with advantage consider the function of a telescope from quite another standpoint. The telescope (apart from special applications to photography, etc.) is an instrument for the purpose of increasing the power of vision—that is, it is an instrument which is an accessory to the eye of the observer using it, and it should therefore be considered in connection with the visual apparatus. From this point of view the telescope becomes simply a device for increasing the focal length of the eye in the ratio of the magnification, and in some cases in addition for virtually shifting the pupil of the eye to a position where it does not restrict the field of vision.

Fig. 1.
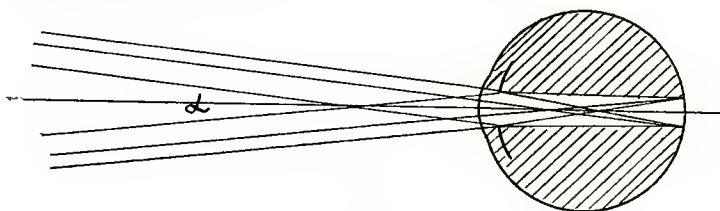
Fig. 1 shows diagrammatically the production of an image on the retina of an object subtending a small visual angle α. The size of the retinal image will be $f\alpha$, where f is the posterior equivalent focal length of the eye. In order to increase the focal length, and hence the size of the retinal image, we may suppose a fluid in contact with the eye of the same refractive index as that of the media of the eye, and a spherical glass plate such as a watch-glass in front (Fig. 2). The fluid will then neutralise the convergence of the cornea, etc.,
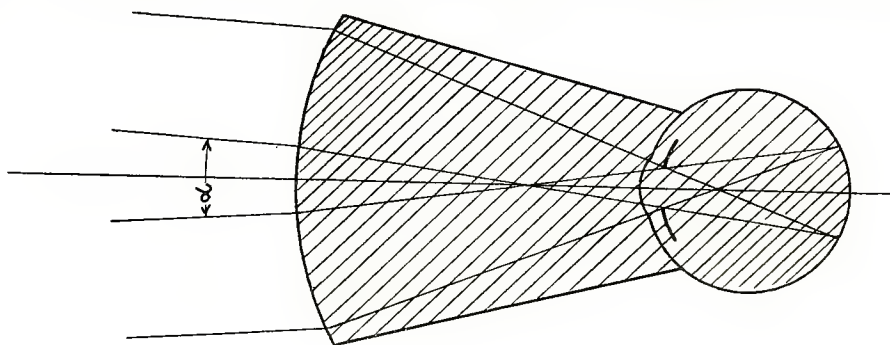
Fig. 2.

and if the front glass be suitably curved the effect will be simply as if the eye had been increased in size, and the image will be proportionally larger. It should be noted, however, that in this case the pupil is not moved, and consequently it is in a different position relatively to the new cornea (the watch-glass) to that in the normal eye. Unless the diameter of the front glass, therefore, is very large, the field of view will be restricted.

In all practical telescopes it is obvious that for the light to be focussed on the retina of a normal eye it must enter the eye as an approximately parallel beam. Whatever, therefore, be the form of the optical system a ray of light entering the instrument parallel to the axis will emerge parallel to it, but may have its lateral distance from the axis altered as in Fig. 3. It may be on the same

side of the axis as the original ray as in positions (*a*) and (*b*), or on the other side as at (*c*) and (*d*). By joining these rays to the intersection of the axis and the retina to which they are focussed, and producing them forwards or backwards, as the case may be, until they meet the original ray we get
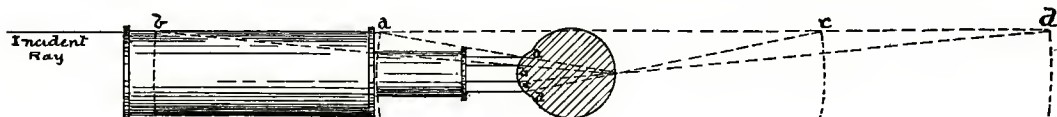

Fig. 3.

the corresponding chief points lying approximately on the second principal plane of the whole system, at which a single refracting surface may be placed to produce the same image. This surface will be convergent, and the image will appear erect if the intersection is in front of the retina, while the equivalent surface will be divergent, and the image will appear inverted if the intersection is behind, as must be the case with all glasses in which the emergent ray is on the opposite side of the axis to the incident ray.   The telescope and eye together may therefore be replaced by an "equivalent eye."

It is also at once evident from the figure that the magnification of any telescope is simply the ratio of the lateral distance of the incident ray from the axis to that of the corresponding emergent ray.   For the magnification is proportional to the ratio in which the length of the eye is increased, and from the geometry of the figure this must evidently be the ratio of the lateral distances.
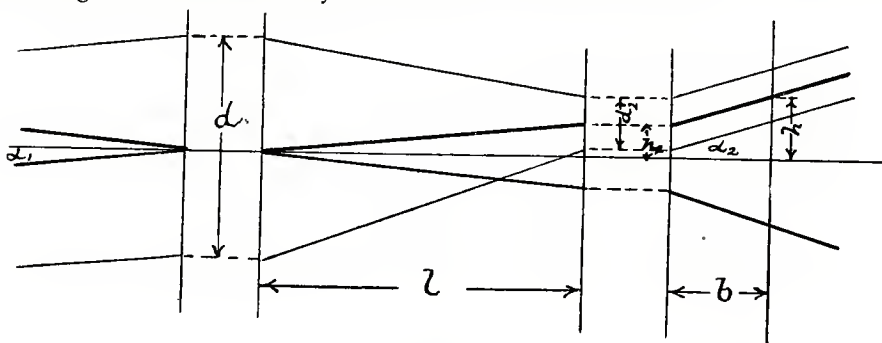
Fig. 4.

General Theory of Telescopes.—Any telescope consists essentially of two components—a converging system or object glass, and an ocular, the latter of which may be either convergent or divergent. Let F_1 be the power or convergence of the objective system, and F_2 that of the ocular, l the distance between the second principal plane of the objective and the first of the ocular, and b the distance from the second principal plane of the ocular to the pupil of the eye.   Let a_1, θ, and a_2 be the angles of obliquity of a ray passing through the system and intersecting the two lenses at heights h_1, h_2, and the pupil at height h.

Then by the method of deviations we have—

$$\tan \theta = F_1 h_1 + \tan a_1,$$

$$h_2 = h_1 - l \tan \theta = - \{(F_1 l - 1)h_1 + l \tan a_1\} \quad . \qquad . \qquad . \qquad . \qquad . \qquad (1)$$

$$\tan a_2 = F_2 h_2 + \tan \theta = - F_2\{(F_1 l - 1)h_1 + l \tan a_1\} + F_1 h_1 + \tan a_1$$

$$= (F_1 + F_2 - F_1 F_2 l)h_1 - (F_2 l - 1)\tan a_1 = F h_1 - (F_2 l - 1)\tan a_1 . \qquad . \qquad (2)$$

where $F = F_1 + F_2 - F_1 F_2 l$, the equivalent convergence of the whole telescope.

$$h = h_2 - b \tan a_2 = - \{(F_1 l - 1)h_1 + l \tan a_1\} - Fbh_1 + (F_2 l - 1)\} b \tan a_1,$$

or $$h = -[\{Fb + F_1 l - 1\}h_1 + \{l - (F_2 l - 1)b\} \tan a_1] \qquad . \qquad . \qquad (3)$$

For normal focussing a_1 and a_2 are both zero, hence $F_1 h_1$ must be zero, and

$$F = F_1 + F_2 - F_1 F_2 l = 0, \text{ from which } l = \frac{F_1 + F_2}{F_1 F_2} = f_1 + f_2 . \qquad . \qquad . \qquad . \qquad (4)$$

Under these circumstances $h_p = -(F_1 l - 1)h_1 = -\dfrac{F_1}{F_2}h_1$,

$$\text{or } \frac{h_2}{h_1} = -\frac{F_2}{F_1} = -\frac{f_1}{f_2} \quad . \qquad . \qquad . \qquad . \qquad . \qquad , \qquad (5)$$

Also $\tan a_2 = Fh_1 - (F_2 l - 1)\tan a_1$ becomes $-\dfrac{F_1}{F_2}\tan a_1$.

$$\text{Hence } \frac{\tan a_2}{\tan a_1} = m = -\frac{F_2}{F_1} = -\frac{f_1}{f_2} = \frac{h_1}{h_2} \quad . \qquad . \qquad . \qquad . \qquad . \qquad (6)$$

or the magnification is the ratio of the apertures of the incident and emergent beams as is well known.

Field of View for Normal Focussing.—The field of view is of great importance, and no formula appears yet to have been published which takes all the factors into consideration. For normal focussing we have from Equation 3

$$h = -\left[\frac{F_1}{F_2}h_1 + \left\{l - \frac{F_2}{F_1}b\right\}\tan a_1\right] = -\left\{\frac{h_1}{m} + (l + mb)\tan a_1\right\}.$$

In order that a ray intersecting the object glass at its edge should pass through the edge of the pupil we have $h = \pm\dfrac{p}{2}$ and $h_1 = \dfrac{d}{2}$, p and d being the pupillary and object glass diameters respectively.

$$\text{Hence } \pm\frac{p}{2} = -\frac{d}{2m} - (l + mb)\tan a_1,$$

from which $2\tan a_1 = \dfrac{1}{m}\dfrac{d \pm mp}{l + mb}$ . . (7)

and $2\tan a_2 = \dfrac{d \pm mp}{l + mb}$ . . (8)

These simple formulæ are of very great utility. If we call $2\tan a_2$ the apparent field of view, mp the magnified pupil, and mb the magnified eye distance, we see that Equation 8 may be expressed by saying that the apparent field of view of any telescope is the ratio of the diameter increased (or diminished) by the magnified pupil, to its optical length increased by the magnified eye distance. A moment's consideration will show that the sign in the numerator depends on whether the ray passes one or other edge of the pupil, and we consequently have—
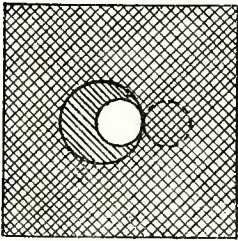
Fig. 4a.

Apparent field of view at maximum illumination $= \dfrac{d - mp}{l + mb}$.

Apparent field of view, extreme $= \dfrac{d + mp}{l + mb}$.

In Fig. 4a the larger circle represents the pupil of the eye, while the smaller indicates the emergent beam, or Ramsden's circle. The illumination is practically constant until the outer edge of the Ramsden's circle touches the edge of the pupil, after which it diminishes the illumination, being proportional to the area of the circle exposed. The limiting angle of view is shown when the inner edge touches the edge of the pupil.

These relations may be readily established in a more simple manner. In Fig. 5 we have the incident ray passing through the optical centre of the object glass. Hence $h_2 = l\tan c$ and $h = l\tan a_1 + b\tan a_2 = (l + mb)\tan a_1$, since $\tan a_2 = m\tan a_1$. But the diameter of the emergent pencil is $\dfrac{d}{m}$ by equation (5). Consequently the distance of the nearer edge of the emergent pencil from the axis is $h - \dfrac{d}{2m}$, and this must not be greater than $\dfrac{p}{2}$.

$$\text{Hence } \frac{p}{2} = h - \frac{d}{2m} = (l + mb)\tan a_1 - \frac{d}{2m},$$

and $2\tan a_1 = \dfrac{1}{m}\dfrac{d + mp}{l + mb}$ as before.

I

It will be seen that in general the limiting field of view is increased by enlarging the object glass or the pupillary aperture, or by reducing $l + mb$.　Where m and b are of the same sign, as in the ordinary
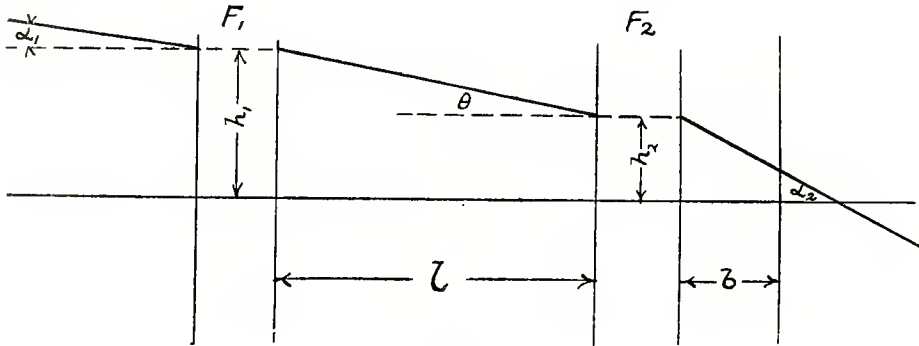

Fig. 5.

Galilean glass, the only means we have of increasing the field of view are to increase the diameter of the object glass, and to make both l and b as small as possible.　Fig. 6 shows how the field of view diminishes with the distance of the eye for a Galilean glass of 5 diameters magnification with a length of 100 mm. and a clear aperture of 50 mm.; while Fig. 7 gives curves for the same aperture with a constant eye distance of 10 mm. for various lengths and magnifications.　The pupillary diameter is taken as 5 mm.　It is interesting to notice that when $\dfrac{d}{l} = \dfrac{p}{b}$ or $l = \dfrac{b}{p}d$ the field is the same for all magnifications as is seen in the figure.
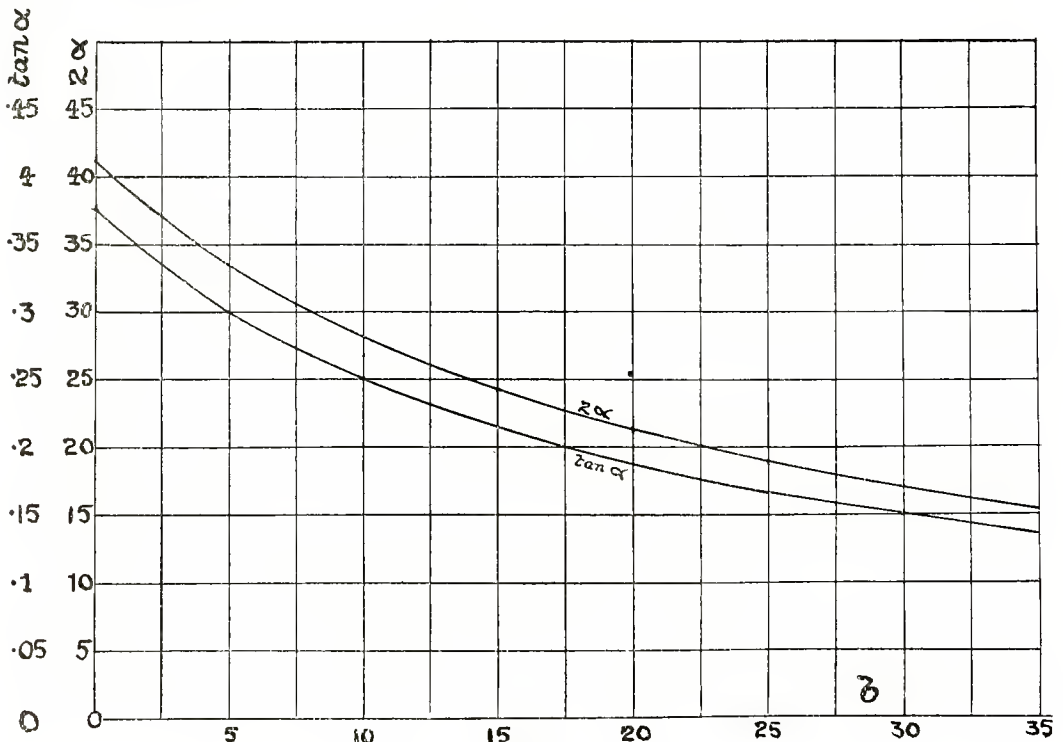
Fig. 6.

With astronomical glasses the magnification m is negative. Hence if $l - mb = 0$ or $b = \dfrac{l}{m}$ the expression is infinite, giving us the position of the "eye point." In this case the field is no longer limited by the pupil, and it is usually bounded by a diaphragm in the focal plane of the eye lens.

We then have, if d' is the diameter of the diaphragm,

$$2 \tan \alpha_1 = F_1 d' = \frac{m-1}{m} \frac{d'}{l} \qquad\qquad (9)$$

$$2 \tan \alpha_2 = F_2 d' = (m-1)\frac{d'}{l} \qquad\qquad (10)$$

Fig. 8 shows the variation in the field of view for an astronomical glass of 10 diameters magnification and a length of 200 mm. The aperture of the object glass is taken as 50 mm., and of the pupil 5 mm., while the effective diameter of the stop is 15 mm. As the eye is withdrawn from the second principal plane of the ocular the apparent field of view increases according to a hyperbolic law, but as soon as it reaches a value of 48° the stop comes into action and limits the field. On further withdrawing the eye, a time comes when the field again contracts.

Illumination of the Image.—From Equation 6 we have $\dfrac{h_1}{h_2} = \dfrac{d_1}{d_2} = m$ where d_1 and d_2 are the diameters of the incident and emergent pencils respectively. Hence $d_2 = \dfrac{d}{m}$. If p is the pupillary diameter, the amount of light received from any object is proportional to the area of the pupil or p^2. The amount of light received in the incident beam will be increased in the ratio of $\dfrac{d_1^2}{p^2}$ and the amount transmitted through the instrument will be $\kappa\left(\dfrac{d_1}{p}\right)^2$ where κ is the ratio of the transmitted to the received light, and may be called the co-efficient of transmission. But since the image is magnified m times, the area over which the same light is distributed is increased in the ratio m^2. Hence the illumination or brightness of the field or image $= \dfrac{\kappa}{m^2}\left(\dfrac{d_1}{p}\right)^2 = \kappa\left(\dfrac{d_2}{p}\right)$ compared with that of the object as is well known, and this may be called the illumination co-efficient or relative illumination of the telescope. If κ were unity, *i.e.* the media were perfectly transparent, and no light were lost by reflection, we should have the illumination co-efficient of the glass $= \left(\dfrac{d_2}{p}\right)^2$. The diameter of the emergent pencil d_2 is frequently termed the Ramsden circle of the telescope, and the relative illumination for perfectly transparent non-reflecting media is therefore expressed by the square of the ratio of the diameter of the Ramsden circle to the effective pupillary diameter for the light considered. In order therefore to get the maximum illumination from a glass of given refractive media we must see that the Ramsden circle is as large as the maximum diameter of the pupil, and that the clear diameter of the object glass should be this diameter multiplied by the magnification. The writer considers that the normal maximum effective diameter of the pupil may be taken as 5 mm., and proposes that any telescope in which the effective diameter of the object glass in mm. equals or exceeds five times its magnification shall be termed a *full aperture glass*. The relative illumination of such a glass will then simply be the co-efficient of transmission of the sum of the media of which it is composed.

In considering this co-efficient of transmission we have two sources of loss, that by absorption and that by surface reflection. The second is given by the well-known expression of Fresnel, which states that at each boundary a fraction $\left(\dfrac{\mu-1}{\mu+1}\right)^2$ of the incident light is reflected. The co-efficient of transmission will therefore be $1 - \left(\dfrac{\mu-1}{\mu+1}\right)^2$ or $\dfrac{4\mu}{(\mu+1)^2}$. For any number n of refracting surfaces of equal index we have transmission $= \left(\dfrac{(4\mu)}{(\mu+1)^2}\right)^n$. In the case of glass having a refractive index of 1·5 the
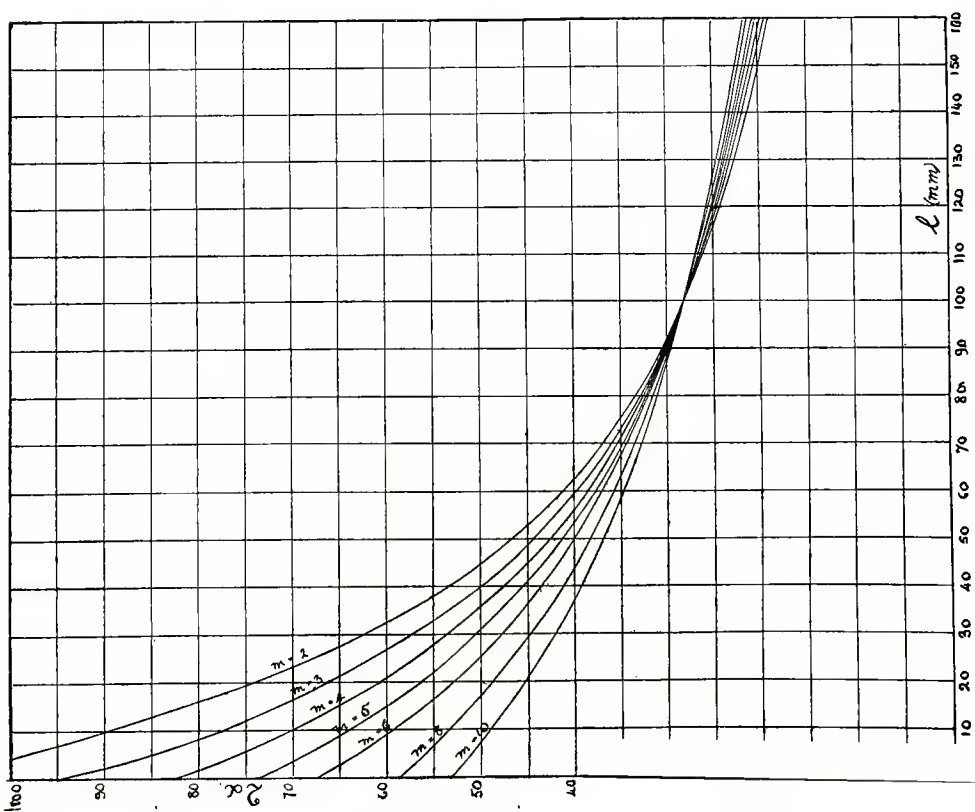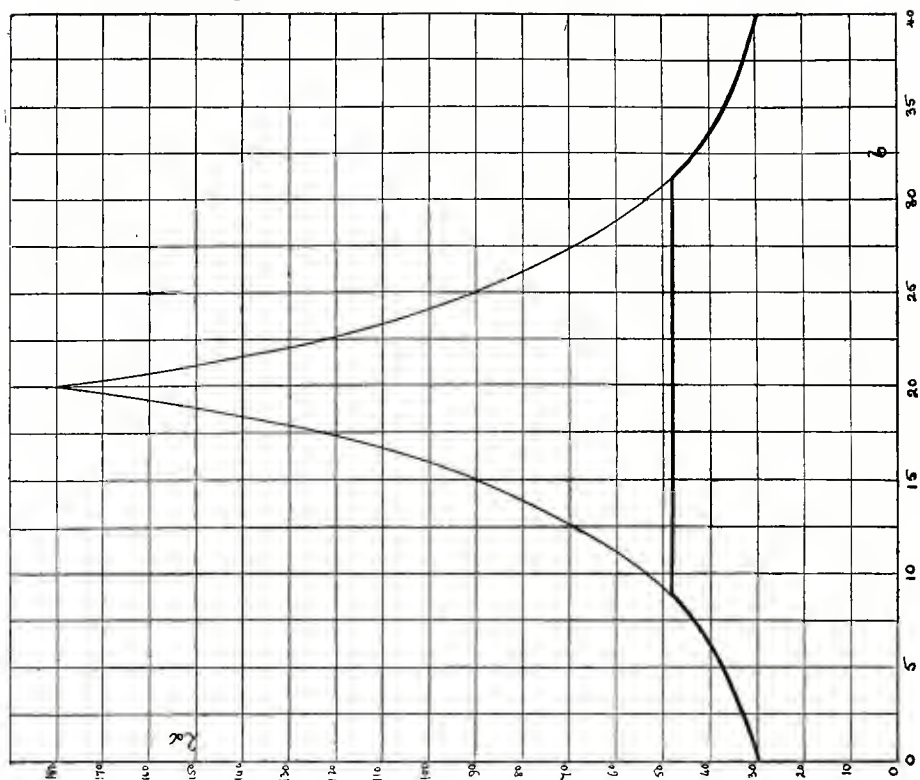
Fig. 8.

Fig. 7.

value of the co-efficient of transmission for a single surface is ·96, and for $\mu = 1\cdot16$ it is ·947. For rough practical purposes it may therefore be taken that each additional lens or prism reduces the illumination by about 10 per cent.

About the loss by absorption not very much is known. Müller[1] has given the following values for the fraction of light transmitted by 10 per cent. of certain Jena glasses.

	μ_D	ν	Co-efficient of transmission for yellow light, 10 cm.	For 1 cm.
Flint O 340	1·5774	41·4	·878	·9847
,, O 102	1·6489	33·8	·829	·9813
,, O 93	1·6245	35·8	·903	·9900
Crown O 203	1·5175	59·3	·872	·9864
,, O 598	1·0152	58·6	·818	·9802

From this it will be seen that for lenses of not more than 1 cm. in thickness the loss by absorption is not very great. It is also interesting to note that there is no definite relation between the refractive index and the absorption.

In the following Table and in Fig. 9 are exhibited some of the properties of glass depending on the refractive index which are of service in designing.

Refractive Index μ	Velocity Constant $N = \dfrac{1}{\mu}$	Critical Angle c $\sin^{-1}\dfrac{1}{\mu}$	Maximum Angle of Obliquity in Glass $\theta = 45° - c$	Maximum Angle of Obliquity in Air $\delta = \sin^{-1}\mu\sin\theta$	Aperture Ratio $\dfrac{d}{f} = 2\tan\theta$	Loss by Reflection $\left(\dfrac{\mu-1}{\mu+1}\right)^2$	Transmitted $1 - \left(\dfrac{\mu-1}{\mu+1}\right)^2$	Transmitted Single Thin Plate	Transmitted 2 Prisms $\left\{1 - \left(\dfrac{\mu-1}{\mu+1}\right)^2\right\}^4$
1·5	·6667	41°49′	3°11′	4°47′	·1674	·0400	·9600	·9217	·8596
1·52	·658	41°9′	3°51′	5°52′	·2056	·0425	·9575	·9166	·8403
1·54	·6499	40°32′	4°28′	6°54′	·245	·0452	·9548	·9116	·8310
1·56	·6415	39°54′	5°6′	7°59′	·281	·0478	·9522	·9065	·8219
1·58	·6333	39°18′	5°42′	9°2′	·318	·0505	·9495	·9016	·8128
1·60	·625	38°41′	6°19′	10°8′	·357	·0532	·9468	·8966	·8039
1·62	·6175	38°8′	6°52′	11°11′	·395	·056	·944	·8963	·7943
1·64	·610	37°35′	7°25′	12°14′	·434	·0587	·9413	·8869	·7848
1·66	·603	37°5′	7°55′	13°13′	·470	·0615	·9385	·8806	·7756
1·68	·5955	36°33′	8°27′	14°18′	·510	·0644	·9356	·8754	·7663
1·7	·588	36°1′	8°59′	15°23′	·550	·0672	·9328	·8702	·7571

Accommodation and Focussing in Telescopes.—In order that the eye may be able to focus the pencil received by it, the convergence of the pencil must lie between certain limits C_1 and C_2, $C_2 - C_1 = C$ being the amplitude of accommodation. For emmetropic persons $C_2 = O$ and $C_1 = C$, while for myopic individuals C_1 and C_2 will both be negative, and in cases of hyperopia C_1 will be positive and C_2 either negative, zero, or positive according to the amount of the defect.

Referring back to Fig. 4, we have—

$$\text{Convergence of pencil entering pupil} = \frac{\tan \alpha_2}{h}$$

$$= -\frac{Fh_1 - (F_2 l - 1)\tan \alpha_1}{\{Fb + F_1 l - 1\}h_1 + \{l - (F_2 l - 1)b\}\tan \alpha_1} \text{ from (2) and (3).}$$

$$= -\frac{F - (F_2 l - 1)D}{Fb + F_1 l - 1 + \{l - F_2 l - 1)b\}D} \cdot \qquad\qquad (11)$$

where D is $\dfrac{\tan \alpha_1}{h_1}$ or the convergence of the incident pencil.

If D is O, or the object is at an infinite distance, $C_1 = -\dfrac{F}{Fb + F_1 l_1 - 1}$ and $C_2 = -\dfrac{F}{Fb + F_1 l_2 - 1}$.

[1] "Jena Glass," Hovestadt. Translated by Prof. J. D. Everett. The values in the last column have been calculated by the writer.
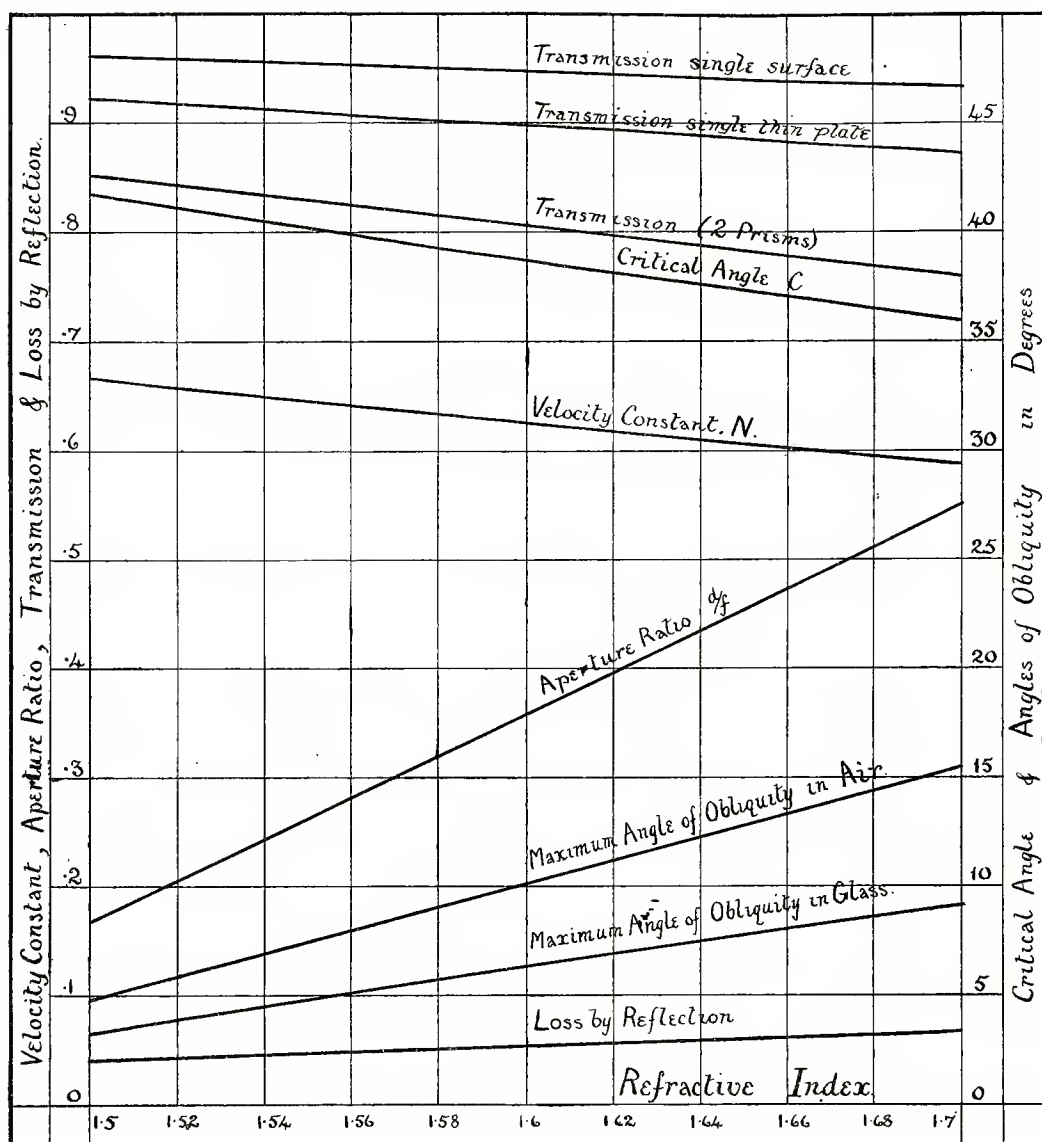
Fig. 9.

Hence $Fb + F_1 l_1 - 1 = -\dfrac{F}{C_1}$ and $Fb + F_1 l_2 - 1 = -\dfrac{F}{C_2}$, from which

$$l_2 - l_1 = \frac{(C_2 - C_1)}{\{C_2(1 - F_2 b) + F_2\}\{C_1(1 - F_2 b) + F_2\}} \qquad (12)$$

When C_1 and C_2 are both small this reduces to

$$l_2 - l_1 = \frac{C_2 - C_1}{F_2{}^2} = (C_2 - C_1)f_2{}^2 \qquad (13)$$

If C_1 and C_2 are in dioptres, and f_2 is in mm., we have $(l_2 - l_1)_{mm.} = \dfrac{(C_2 - C_1)f_2{}^2}{1000}$, a formula which is useful in calibrating eye-piece focussing adjustments.

From (11) we have—

$$D = -\frac{(Fb + F_1 l - 1)C + F}{\{l = (F_2 l - 1)b\}C - (F_2 l - 1)} \qquad (14)$$

For normal focussing $F = O$, and if at the same time C is very small we have—

$$D = \frac{F_2 l - 1}{F_1 l - 1} C = \frac{C}{m^2} \qquad\qquad (15)$$

showing that the amplitude of accommodation with the glass is reduced in the ratio of the square of the magnification. For an emmetropic person having a near point at a distance c, the nearest point that can be seen with a normally focussed glass will be at a distance $d = m^2 c$.

The simple relations (13) and (15) can be obtained at once from Newton's relation $pq = f^2$ where p and q are the conjugate distances measured in the focal plane. For p represents the movement of the image from the first focal plane, if the object moves from infinity to a distance q from the second focal plane. If q is large $\frac{1}{q_1}$ may be taken as C_1, and $\frac{1}{q_2}$ as C_2, and we have $p_1 = f_2^2 C_1$, and $p_2 - p_1 = l_2 - l_1 = f_2^2 (C_2 - C_1)$ as before. Also, if d is the distance of an object from the object glass, we have $p_1 = f_2^2 C_1$, and $d - f_1 = \frac{f_1^2}{p_1} = \frac{f_1^2}{f_2^2 C_1}$. If C is small $\frac{1}{d-f} = D = \frac{f_2^2 C_1}{f_1^2} = \frac{C_1}{m_2}$ as above.

The movement of the eye-piece consequent on focussing a distant object is, evidently, given by—

$$\Delta l = \frac{f_1^2}{d - f_1} \text{ or } \frac{f_1^2}{d} = \frac{m^2}{(m-1)^2} \frac{l^2}{d} \text{ if } d \text{ is large} \qquad (16)$$

The magnification and field of view of the telescope will of course be modified by focussing, and the Equations 2 and 3, combined with the conditions for accommodation, will give them if required.

Calculation of the Aberrations.—In calculating the aberrations of telescopes, the writer has employed the methods of Von Seidel with satisfactory results. Two points, however, should be borne in mind in applying Von Seidel's equations to telescopes. In the first place, the separation between the objective and ocular systems is comparable with the focal length of the former, which, with large compensatory aberrations, makes the approximation not sufficiently exact. Secondly, as is well known, the first of

Von Seidel's expressions (that for spherical aberration) may be written $\Sigma\, h \left(\dfrac{\sigma_+ - \sigma_-}{N} \right) \left(\dfrac{\sigma_+}{\mu_+} - \dfrac{\sigma_-}{\mu_-} \right).$

Now, in order to obtain the coma, astigmatism, and distortion, we have to multiply each term in this summation by the quantity U given by the expression $\dfrac{N}{h(\sigma_+ - \sigma_-)} + \Sigma\, \dfrac{t'}{h_+ \, h_-}.$ In the case of light intersecting a surface close to the axis, or nearly normally or both, as will generally be the case in the field lens of the ocular, the expression for the spherical aberration at that surface will be very small,

Fig. 10.

while U will become very large. The terms relating to astigmatism, curvature, and distortion, in which the second and third powers of U enter, may therefore beco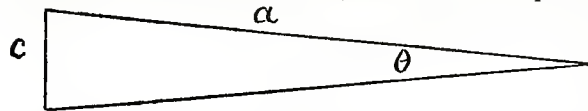me quite ambiguous in such cases. We may get over this difficulty in actual design by arranging our system so as to avoid such indeterminate surfaces, or we may adopt other devices which cannot here be described. Of course we can always fall back upon exact trigonometrical calculations.

Stereoscopic Effect in Binoculars.—The stereoscopic effect or perception of solidity obviously depends upon the distance of the object from the observer, and upon his inter-pupillary distance. In any triangle approximately isoceles (Fig. 10) in which the apical angle θ is small, we have $\theta = \frac{c}{a}$, and consequently, $\Delta\theta = \frac{c}{a^2}\Delta a$. But if the object is viewed through a telescope of magnification $\Delta\theta' = m\Delta\theta$ where $\Delta\theta'$ is the variation of the apical or convergence angle as measured by the eye.

Consequently, if an object at any distance has a depth Δa, and it is viewed by an individual having an interpupillary distance c, we shall have $\Delta\theta = \frac{c}{a^2}\Delta a$, and if he looks at the same object through a binocular having a magnification m and a width between object-glass centres of C', $\Delta\theta' = m\frac{C'}{a^2}\Delta a$.

Hence $\frac{\Delta\theta'}{\Delta\theta} = m\frac{c'}{c}$, and if we choose to define the stereoscopic power of a binocular as the relative angular displacement consequent upon a given displacement in the line of sight, we have

$$\text{Stereoscopic power} = m\frac{c'}{c}.$$

This subject has been investigated by Mr Cheshire [1] from the standpoint of the stereoscopic range with similar result.

<div align="center">PART II.</div>

WE now come to the practical construction of the various glasses, and a few words will be devoted to each of the principal forms—the Galilean, the Terrestrial, and the Prism glasses.

Galilean Glasses.—These glasses are undoubtedly the simplest both in theory and construction, and they have the further advantages of great illumination combined with lightness and shortness. In fact, glasses of this kind have only one defect, the smallness of the field of view, but this defect has been so marked as to cause them to fall into disfavour, and to give place to the prism glass, which has exactly the opposite characteristics. An important feature of the Galilean glass is that the ocular being a negative lens has the opposite chromatic effect to the object-glass, and may therefore be made to correct part of the chromatic aberration of the former.

In consequence, Galilean glasses are made in two forms— the first in which the eye-lens is a single concave lens, and the object-glass is an under-corrected positive combination; while in the second both object-glass and eye-lens are completely achromatic in themselves. These two forms are known as six-lens
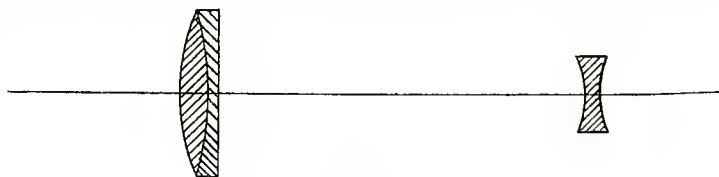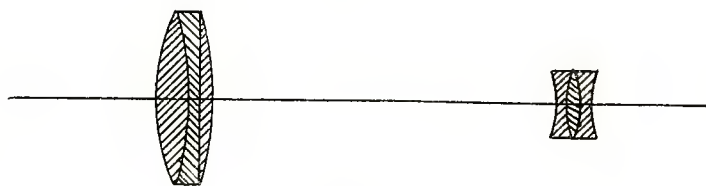

Fig. 11.

Fig. 12.

and twelve-lens glasses respectively, owing to the fact that in the latter the object-glass and eye-lens are each made triple, presumably for the protection of the flint from the atmosphere.

Figs. 11 and 12 show typical sections of a six- and a twelve-lens glass. As far as central definition is concerned, there is nothing to choose between the two types, but it is of course impossible to correct chromatic aberration for the centre and edge of the field simultaneously without making both the object-glass and eye-lens achromatic in themselves.

Chromatic Aberration.—To make the glass achromatic in itself at the centre of the field all that is necessary is that the linear chromatic aberrations of the object and eye-lenses should be equal and opposite. For thin lenses this gives us the relation $\Delta f_1 + \Delta f_2 = 0$, or $\frac{\Delta F_1}{F_1^2} + \frac{\Delta F_2}{F_2^2} = 0$. If each of the lenses are single we have $\frac{f_1}{v_1} + \frac{f_2}{v_2} = 0$, from which $\frac{v_1}{v_2} = -\frac{f_1}{f_2} = m$, where v_1 and v_2 are the "efficiencies" or

[1] *British Optical Journal*, vol. iii. p. 16.

"achromatic refractivities" of the glasses. If ν_1 and ν_2 are equal, $f_1 + f_2 = O$ and m can only be unity, but if a pair of glasses are selected in which ν_2 is twice ν_1, a Galilean glass can be made up with single lenses of a power of two diameters without any central colour. The edge colouration is of course large.

The general expression for central achromatism of any combination is $\Sigma h \dfrac{\overset{\sigma}{+} - \overset{\sigma}{-}}{N}\left(\dfrac{\overset{\Delta\mu}{+}}{\overset{\mu}{+}} - \dfrac{\overset{\Delta\mu}{-}}{\overset{\mu}{-}}\right) = O$

If the separate component lenses are of small thickness, this reduces to $\Sigma h^2 \Delta F = O$, corresponding with the second of the above equations. Where the O.G. and eye-lens are both thin combinations, this gives us $h_1^2 \Delta F_1 + h_2^2 \Delta F_2 = O$, or $m^2 \Delta F_1 + \Delta F_2 = O$, showing how much more important the correction of the object-glass is than that of the eye-piece. But for edge achromatism we must have constancy of the magnification $m = -\dfrac{F_2}{F_1}$ for all colours, from which $F_2 \Delta F_1 - F_1 \Delta F_2 = O$ or $m \Delta F_1 - \Delta F_2 = O$. For simultaneous correction of central and edge colour therefore both ΔF_1 and ΔF_2 must be O, or the objective and eye-lens must each be achromatic in themselves, as is well known, and which is carried out in the twelve-lens glass. There seems no reason, however, now, why good glasses should not be made in which the lenses are double instead of triple.

In order to compensate for the chromatic aberration of the eye the equations reduce to—

Central $m_2 \Delta F_1 + \Delta F_2 = -\cdot7$ dioptre approximately.

Oblique $m \Delta F_1 + \Delta F_2 = O$.

The spherical and oblique aberrations can be worked out by the ordinary Von Seidel formulæ without much difficulty.

Angle of View.—Since the Galilean glass has so many advantages, it is worth while for manufacturers to endeavour to improve its sole defect, that of narrowness of field of view. In prism glasses, as ordinarily made, the apparent angle of view does not exceed $45°$ or $50°$, and from the curves given in Fig. 7 it will be seen that it is possible to attain a limiting angle of about $30°$ to $32°$ with a magnification of 6 to 8 diameters in a Galilean glass, of about 80 mm. length. Of course the illumination falls off greatly at the edge, but this is not of vital importance. From the formula $2 \tan a_2 = \dfrac{d + mp}{l + mb}$ it is evident that to improve the field there are three courses open to us. We may make d as large as possible (55 mm. is about the limit in binoculars), reduce l by increasing the convergences of both lenses, and make b as small as possible. By making the eye-lens in the form of a thick meniscus it might be possible to bring the nodal point of emergence nearer the eye and thus to further extend the field. All these things will require considerable care in working out, but there can be little doubt that the Galilean glass can be very greatly improved, as the writer has found from actual experience.

Terrestrial Telescopes.—The terrestrial glass is equivalent to a telescope of the Galilean form in which the nodal point of emergence of the ocular is pushed out well beyond the eye-lens. For this purpose it is only theoretically necessary to employ an ocular consisting of two convergent lenses separated by a distance greater than the sum of their focal lengths. From the formulæ $f = \dfrac{f_1 f_2}{f_1 + f_2 - d}$ and $n = -\dfrac{f_2 d}{f_1 + f_2 - d}$ where f is the equivalent focal length and n the distance from the second nodal point, it is evident that by making d greater than $f_1 + f_2$ the combination has a negative focal length, and that the nodal point is outside the system. The difficulty in realising this idea in practice is that it necessitates large lenses in the ocular of deep curvature as will be seen from Fig. 13. Nevertheless, the writer believes that it has been found possible to make a satisfactory telescope on this basis, with the advantage over ordinary terrestrial glasses of shorter length and less loss of light by surface reflection. The ocular, however, that has been most favoured is a four-lens one, represented in Fig. 14, the course of the light being represented in the diagram.

At the present time the terrestrial glass is somewhat out of favour, and it can never be very

convenient as a portable or binocular glass, but with improved design of the ocular system much may perhaps be done.
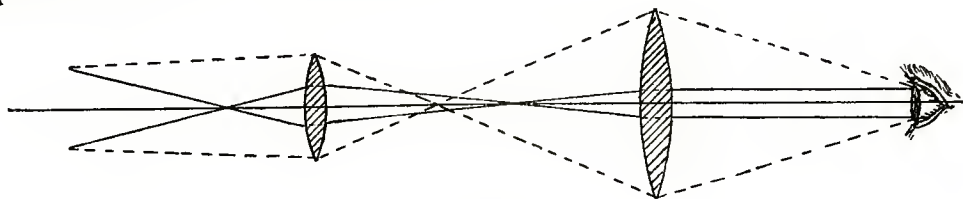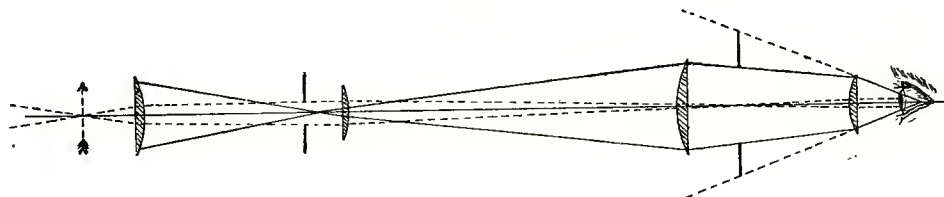
Fig. 13.

Fig. 14.

Prism Glasses.—These glasses are of the greatest interest at the present time, and require some consideration, as very little detail concerning them has been published. Invented in 1853 by Porro, they were not made use of at that time owing to the want of uniformity of the glass then procurable, and it remained for Messrs Zeiss in 1895 to revive the idea, and to carry it out with the success with which everyone is familiar.

As is well known, the principle of these glasses is that of the ordinary astronomical telescope, erection of the image being obtained by totally reflecting prisms between the objective and ocular. In consequence, a large field of view can be obtained with a very simple lens system, and it is in this respect that the prism glass has its superiority. In all other respects the advantage lies with the Galilean form.

The typical form of prism glass is that manufactured by Messrs Zeiss and many others, and shown in Fig. 15.
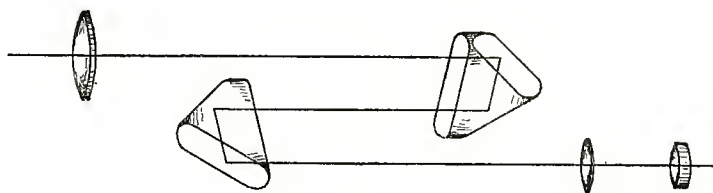
Fig. 15.
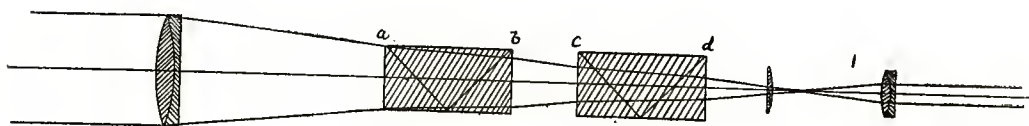
Fig. 16.

It will be seen that so far as the optical distance travelled by the light is concerned, this arrangement is equivalent to the development in Fig. 16, and this illustrates the other advantage of the prism glass, that of utilising a long telescope with weak lenses and flat field in a short body. It is essential for the satisfactory operation of such a telescope that the prisms should be of the most homogeneous and transparent glass, that their surfaces should be perfectly plane and inclined at the exact angles to

one another, and lastly, that they should be fixed in the body in the correct relationship to one another. Great care has therefore to be taken in the manufacture and setting of these prisms, the latter being generally effected by mounting the prisms in seats cut on platforms in the body of the glass.   On the Continent, machine tools are made which automatically finish the body at one setting.   One or two other methods of mounting the prisms will be described later.
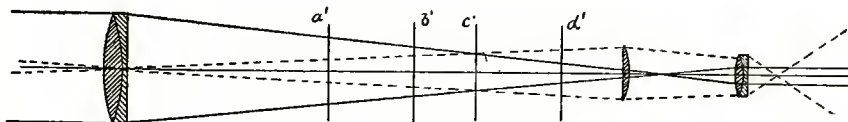

Fig. 17.

It will be obvious that the magnification and field of view of a prism glass will be identical with that of an astronomical telescope having the same lens system, provided that the marginal rays are not cut off by the prism.   When we come to the question of illumination, however, we find that these glasses are markedly deficient, three difficulties presenting themselves in the way of increasing it.

Illumination in Prism Glasses.— From Fig. 16 which represents the development of an ordinary prism glass, and Fig. 17 in which the prisms are represented by their equivalent thicknesses, the first
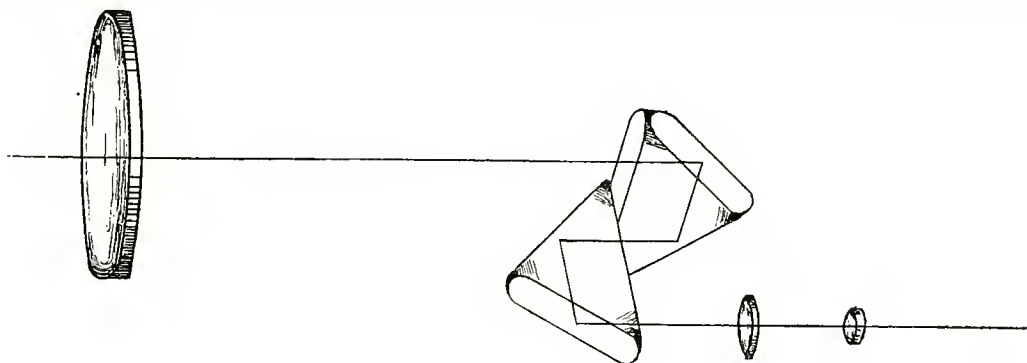

Fig. 18.

two of these difficulties are immediately apparent.   The size of the object-glass that can be effectively employed is restricted by the aperture of the cone of light that can pass through the prisms.   In order to avoid unduly heavy and bulky glasses, the aperture of the prisms cannot much exceed 15 mm., and this restricts us to an objective of about 23 mm. effective aperture.   In the second place, in comparison with the ordinary astronomical glass we have four additional surface reflections.
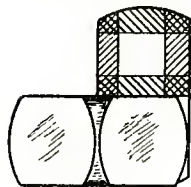

Fig. 19.

Towards the end of 1900 the writer was asked by Messrs Aitchison & Co., who had noticed the unsatisfactory illumination of prism glasses, to look into the matter with the object of improving the illumination.   With the foregoing in mind, the obvious step was to bring the prisms close together as in Fig. 18, and to get rid of two of the surface reflections by cementing the prisms into contact.   The third difficulty then arose that with ordinary prisms the critical angle was not much less than 45°, and in consequence, any increase of the O.G. diameter beyond a certain point without increasing the length of the glass would be ineffective owing to the light not being reflected.   In fact, if two ordinary prisms are looked through with the naked eye an appearance as in Fig. 19 is seen, a square patch of full brightness being visible, with a dark field surrounding it.   With a refractive index of 1.5 the critical angle is 41° 49', and the obliquity of the light in the glass therefore cannot exceed 3° 11' without passing the

angle of total reflection.   This corresponds to an angle of $4^{''}$ 47' in air giving a ratio of $\frac{d}{f}$ of .167, which must not be exceeded .   For a glass of a magnifying power of 10, corresponding to a "full aperture" of 50 mm. this would necessitate a focal length of 300 mm., which, as the prisms are now close together, would be unallowable in a portable glass.   Two remedies were obvious, either the refractive index of the glass might be increased to give a lower critical angle, or the equivalent focal length of the objective might be increased without lengthening the glass, by interposing a divergent lens between it and the first prism, preferably as near it as possible.   The first device being more simple, was tried immediately, and found to give satisfactory results, a very transparent light flint glass of refractive index about 1.575 being used for the prisms.   It is important both on account of transparency and of surface reflection, to keep the refractive index as low as possible compatible with the correct critical angle, and in the curves in Fig. 9 will be seen the relation of the obliquity and ratio of $\frac{d}{f}$ to the refractive index, enabling the glass required for the prisms for any aperture ratio to be found at a glance.   The loss of light (apart from absorption) is also shown.   It has been urged by some that the use of glass of higher index is objectionable, as it is not so transparent, but as by this device we may increase the light entering the objective threefold, and the loss could not be increased by more than 10 per cent., the objection is of no value.   The most absurd criticisms have been made concerning the use of the divergent lens.   The possibility of increasing the luminosity in this way has been on the one hand flatly contradicted, while, on the other, statements have come from individuals claiming to have used *convex lenses* for the purpose or a divergent lens *between the prism system and the eyepiece*.

The increase of the illumination of a prism glass is, of course, attended with very considerable difficulties, in the design of the optical system.   The size of the confusion disc in the focal plane of the objective is proportional to the fourth power of the aperture and the inverse cube of the focal length.   As the first is approximately doubled, and the second halved, the problem of correcting the spherical aberration is not a simple one, and justifies the statement before made that the same care in design is needed as for a photographic or microscopic objective.   Anomalous glasses are used to reduce the otherwise great curvature of field.   Numerous careful designs have been worked out, with the result that now the definition at the same aperture (not relative aperture) appears to be equal to that obtained in good glasses of ordinary focal length.

Where the illumination of the object is good, this aperture is all that is necessary, but when the light is at all dull the full aperture may be used with the greatest advantage, as the small amount of spherical aberration thus introduced is not noticeable in a dull light.   To enable the glass to be always used to the best advantage the writer suggested the use of iris diaphragms between the objective and prisms, and although the use of such diaphragms has been criticised, he is satisfied that they are of considerable value.   The ability to vary the amount of light received by the eyes is most restful, and enables the eye to concern itself only with the detail.

The mechanical design and construction of the glass has been mainly in the hands of Mr Kendall who has solved a number of difficult problems in an original and satisfactory manner.   The two body tubes are cast together in one piece to secure rigidity, and the object glasses more simultaneously in these tubes by a scroll motion controlled from the central focussing pillar through the cross-bar, thus eliminating the slightest possibility of the tilting, so liable to take place in ordinary sliding focussing motions.   A second milled head on the central pillar controls the two iris diaphragms, so that either the focussing or light regulation can be done by one hand.

To enable the interpupillary distance to be adjusted the prisms and eyepieces are mounted in triangular boxes, each strongly fitted to the main tube of the body, and these boxes are so geared by toothed segments in the upper cross arm as to move symmetrically together.   Eyepieces of the usual form are employed, though of short focal length, and containing anomalous glass, and these are each mounted so as to be capable of independent focussing.   To enable the prisms to be conveniently adjusted they are mounted in special clips attached to a pillar at the side of the prism box.

From the above description and the annexed illustrations (Figs. 20 and 21), the construction of the glass will be sufficiently clear.

Different Forms of Prism Glasses.—Space will not permit of detailed description of other forms, but the diagrams adjoining Fig. 22, *a* to *h*, give the principal arrangements of prisms in use. Besides the right angle prism the chief device is that of the penta-prism or roof prism. This has the advantage of enabling the awkward triangular body to be avoided, but is difficult to make, and requires one surface to be silvered, which is always to be avoided where possible.

In Fig. 22 *a* is the arrangement most frequently employed, and *b* that employed for the Aitchison large aperture glasses. The arrangement shown in *c* enables a somewhat larger beam to be obtained for
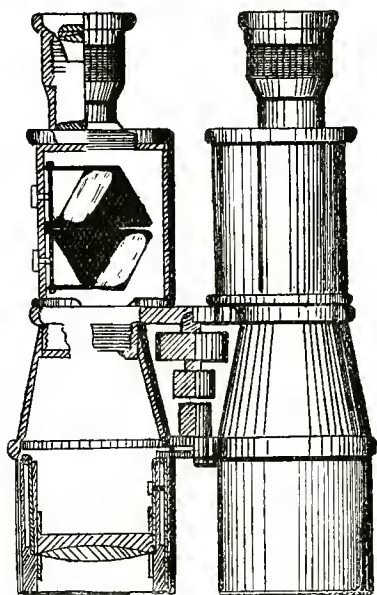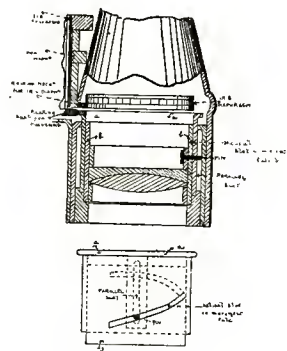

Fig. 20.

Fig. 21.

a given prism space, and the writer has proposed to use the form of prism shown at *d*, two such prisms being cemented together. In *e* is shown the pentaprism arrangement, while *f* and *g* show arrangements of prisms suitable for glasses of great stereoscopic power, or for use in range-finders. Messrs Dallmeyer have made use of four separated prisms, as in *h*.

A somewhat interesting form of glass has just been patented in this country by Messrs Pütz, of Wetzlar, the optical principles of which are illustrated in Fig. 22*i*. The prisms are in this case of the form of truncated tetrahedrons, and it is claimed that the optical distance traversed in the prisms is smaller. By combining two such sets of prisms they arrive at a form of binocular in which the objectives and oculars are opposite one another, but the right hand ocular receives light from the left O.G., and *vice versâ*. The form of the prisms in this case appears to be the same as *d*. The arrangement is very neat and compact, but it would be very interesting to know the result of the pseudoscopic effect thus introduced.

Adjustment and Testing.—Little need be said on these points, as they have been already dealt with elsewhere. The principal adjustment required in a prism glass is the setting of the prisms so that the axes shall be exactly parallel, and that objects are truly erect. Although this may be done by ordinary inspection it is a very long and tedious process, and the writer therefore devised an apparatus (Fig. 23) which enables these adjustments to be made with great facility. A brief description of this apparatus
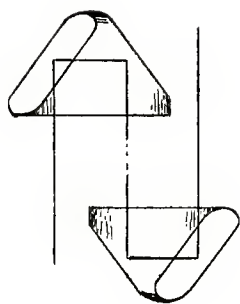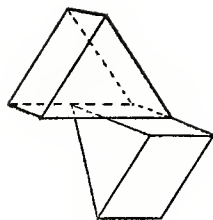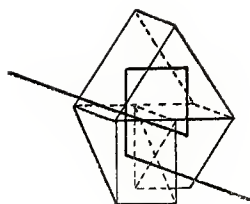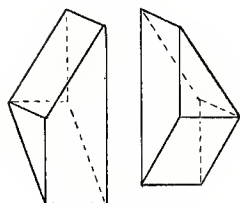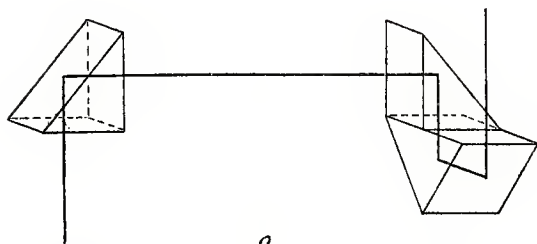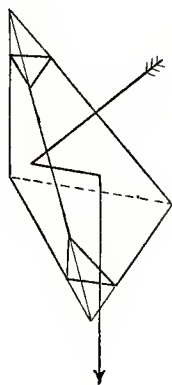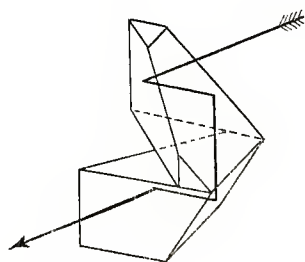
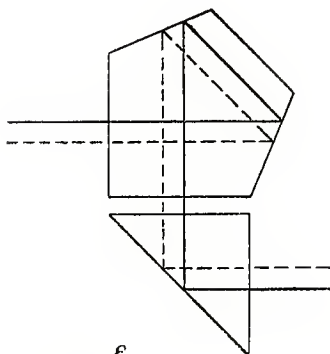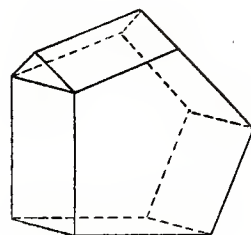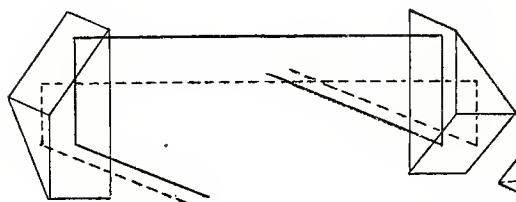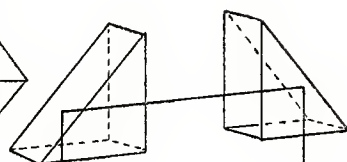Fig. 22.

has already been given at the Optical Society, but a diagram is here given to show its working. As previously explained, the apparatus consists of two tubes the function of each of which is to project a bright image of a cross focussed at infinity. These two tubes are placed side by side, the distance between them being capable of adjustment to suit binoculars of different widths, and in front is placed a convex
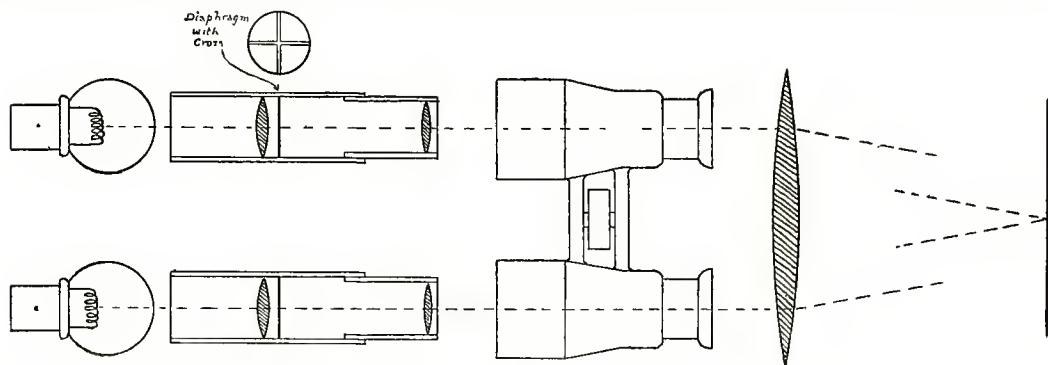
Fig. 23.

lens of large aperture and a screen at its focus. The two projecting tubes are so set that the two images are exactly superposed when no glass is inter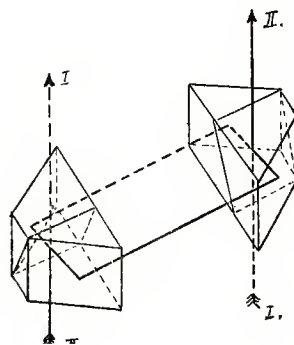posed between them and the lens. On placing the glass to be tested in front of them, the crosses are again seen either magnified or diminished, and the prisms may be easily adjusted for the crosses to be coincident. The magnification of the glass also can be directly read off.

The testing of the aberrations of telescopes is best done on a star in the well-known manner. For small glasses the writer has used a long focus collimater with good results.
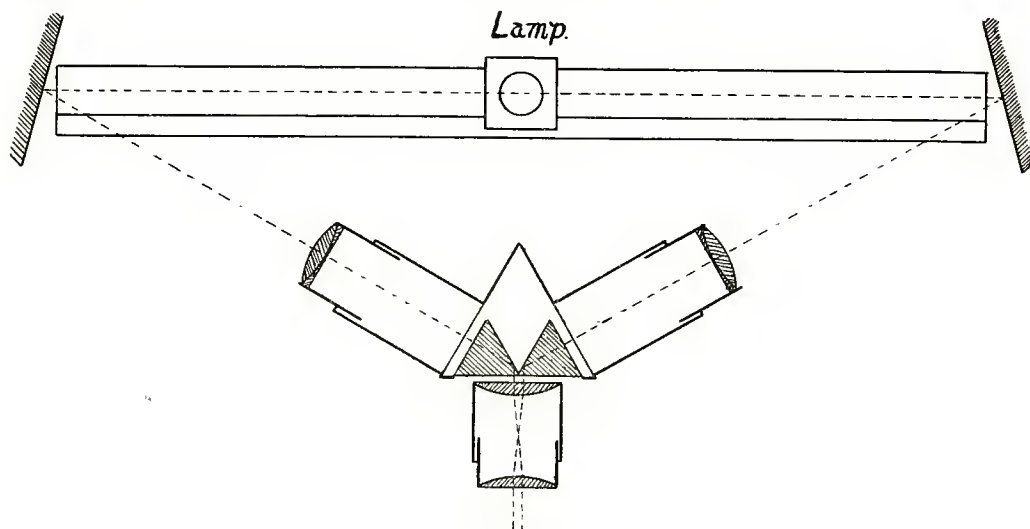
Fig. 24.

Testing the illumination.—In order to measure the relative illumination of glasses, the writer has devised a special photometer shown in Fig. 24. Two screens are illuminated by a source of light which can move between them, so as to vary the illumination falling on each. The photometer head, shown to a larger scale than the bench, consists of a box in the form of an equilateral triangle, and contains two 60° prisms with their edges touching, and cemented to

a plain slip of glass. In front of this is the observing tube, with an eyepiece focussed on the line of junction of the prisms, while two tubes, each with lenses,[1] focus, the infinitely distant objects by total reflection on the same plane as the eyepiece. On looking through the eyepiece, therefore, distant objects at 120° can be simultaneously seen, and if this is placed so that it is symmetrical with the bench and screens, the two halves of the field will appear equally illuminated, with the source of light at the central position. On interposing any telescope or plate of glass between one of the tubes and the screen, and moving the light so as to restore balance, the relative illumination will be given immediately on a direct reading scale on the bench, and this can be checked by moving the glass over to the other side, as the arrangement is absolutely symmetrical.

A photometer for a similar purpose has been devised by Krüss, but the writer has not seen details of it, and does not know the principle utilised. The writer's form has only just been completed, and measurements have not yet been made. He hopes, however, to have an opportunity shortly of measuring the absorptions of various glasses. This apparatus, as well as the collimating device, has been made by Messrs Aitchison & Co.

Testing the Magnification.—Although this is an extremely simple matter, the methods generally employed are somewhat crude. Either it is estimated by the double vision method, or by the dynameter, which gives the diameter of the Ramsden's circle. The former is difficult of accurate performance, while the second may be misleading. A projection method of demonstrating the magnification was described by the writer in 1902, but the same thing may be very conveniently carried out visually by having a small astronomical glass of low power, or even arranged to reduce instead of magnifying. If a scale is placed in the focal plane of the eyepiece of this glass, and it is focussed on a distant object, it is only necessary to observe the number of scale divisions covered by the object, with and without the telescope, to be tested. By suitably arranging the scale the field of view may be simultaneously read off directly, provided that the instrument is arranged with a diaphragm of the same diameter and in the same position as the pupil of the eye.

In conclusion, the writer hopes that this hurriedly written paper, although by no means exhaustive, may be of some service as a stop-gap until something more complete is available. He also ventures to hope that manufacturers will come forward and give to the scientific bodies papers relating to practical optical matters, which may help to bring our optical text-books a little more up to date.

The President (Dr GLAZEBROOK) congratulated Dr Drysdale on his excellent account of the instruments included in his paper, and specially noted the excellent results that had followed the practical application of sound theoretical work.

He was specially interested in the descriptions of the instruments used for testing and adjusting.

By the generosity of Mr Aitchison, the National Physical Laboratory now owned the apparatus for testing parallelism of axes, and were using it in testing binoculars.

The photometer, for testing the absorption in binoculars, described by Dr Drysdale, was certainly a very simple piece of apparatus; his laboratory had just received, and were exhibiting, a photometer bench, made by an English firm on the principle used by Krüss; it was much more elaborate, but had the advantage that it could be used for other photometric measurements as well as for testing absorption.

Mr H. F. TAYLOR referred to an example of a two-lens erecting eyepiece which had passed through his hands some time ago. It was, he thought, similar to that sketched by Dr Drysdale.

Dr DRYSDALE, in reply, said that he was much obliged to Dr Glazebrook for his remarks. With reference to the photometric examination of field-glasses, he had been aware of the existence of the

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[1] Each of these lenses and the observing eye-piece must have the same focal length, in order that the whole apparatus shall not magnify or diminish the image given by the glasses under test, otherwise the diameter of the Ramsden circles would be affected.

Krüss photometer, but had had no opportunity of obtaining details concerning it.   He was interested to hear of Mr Taylor's two-lens erecting eyepiece, and hoped that instrument-makers would devote some attention to new possibilities in this direction.

A subsequent examination of the Krüss bench had led him to the opinion that it is hardly suitable for the examination of telescopes under practical conditions.   According to the information he had received, such tests are proposed to be made with this bench by the device of attaching two similar telescopes end to end, so as to eliminate the magnification of either, and to treat the combination as an absorptive screen.   Apart from the difficulty of making such tests, and the objection to the employment of two glasses instead of one, it might be questioned whether the method was strictly valid.   In the author's form the telescope is tested under absolutely normal conditions, and the symmetry of the arrangement enables the readings to be obtained with the telescope in front of either screen in a few minutes.   The whole arrangement is also very simple, and inexpensive to construct.

THE PRESENT POSITION OF PHOTOMETRIC MEASUREMENTS.

By A. C. Jolley.

Every branch of scientific enquiry is centred round some fundamental phenomenon, and usually develops along the following directions :—

    I. The investigation of methods of producing the phenomenon.

    II. The measurement of quantities directly connected with it.

    III. Its applications to the elucidation of scientific or industrial problems.

The fundamental phenomenon in Optics is light, and although the science of Optics has developed to great perfection along the last direction mentioned above, the first and second courses are by no means so perfectly developed, particularly by opticians in this country.

This lack of attention to the production and measurement of light is probably due to the fact that the introduction of artificial illuminants is comparatively recent, and the early problems were almost entirely attacked by the aid of light from natural sources.

The production of light by artificial means opened up a new field of enquiry.   It became necessary to know the cost of producing a given quantity of light, and therefore the subject of Photometry has subsequently been largely developed by the electrical and gas engineer, while many important optical problems remain either untouched or imperfectly treated, due entirely to a want of specialised knowledge on the part of the optician in this particular direction.

The subject of Photometry has, so to speak, been developed out of its own country of recent years, and bristles with problems which call for special optical treatment, for, unlike most other scientific measurements, not only have we to deal with a physical quantity, but also a physiological phenomenon, and our ultimate measuring instrument must be the human eye.   It is here that the optician with his specialised knowledge of that organ and also of optical appliances finds much important work awaiting investigation.

Although photometric measurements are comparatively simple, it is surprising how difficult it is to obtain accurate and consistent results.   In fact, C. H. Sharp in America has put the limit of commercial accuracy at two per cent., a figure that has been repeatedly confirmed by the author's own measurements extending over a number of years.   Higher accuracy can only be attained by taking extreme precautions.   It will be seen that there must be considerable room for improvements in the photometric process, and I have therefore endeavoured to collect and bring before you the

present aspects of the subject, in the hope that it may stimulate opticians to give their attention to the important optical problems which it contains.

There would seem to be two main causes for the unsatisfactory state of these measurements of luminosity.

Firstly.—The difficulty of standardising and using the present working standards of light which in nearly every case require laborious determinations of the condition of the atmosphere with respect to pressure, humidity, and impurity, or of delicate measurements of electrical pressure.

Secondly.—The fact that all our measurements must ultimately be referred to the eye as the standard piece of apparatus. Hence our measurements will depend upon a physiological sensation which may, and does, vary with fatigue, alteration of wave-length, intensity, and pupillary opening.

The condition of health of the observer is also an important factor, and has great influence upon the accuracy. A photometric measurement is therefore not only the determination of an objective quantity, but is also subjective, since it must depend upon this physiological sensation.

Let us here examine the objects of photometric measurements.

Firstly.—We may wish to determine the intensity of a given source of light, expressing it either as the mean intensity given out in a horizontal direction or as the mean intensity falling on a spherical surface, having the source as its centre.

Secondly.—We may wish to determine the amount of light reflected, transmitted, or absorbed by a given substance or optical system.

Thirdly.—We may wish to determine the illumination at a given place due to a distant source.

Fourthly.—We may wish to examine the distribution of light from a given source.

Standards of Light.—It will be obvious that before any of the above objects can be achieved we must institute some unit, and create a standard representing that unit.

Now it will at once be seen that since our measurements will depend, as mentioned above, upon a physiological sensation, we cannot have an absolute standard of light, and therefore our unit of light cannot be expressed in the C.G.S. system.

In the early history of the subject it was natural for investigators to turn to the most common source of artificial light for their unit, and hence arose the term candle-power, and the endeavours to standardise candles to form a reproducible unit.

In England, Germany, and France, standard candles have been produced, having specially-formed wicks, and designed to burn a definite weight of combustible per hour. But exhaustive researches have shown that these candles do not form suitable standards, since their light depends upon flame height, position of the wick in the flame, rate at which the combustible is consumed, and many other variables which are to a certain extent beyond the control of the observer, and which lead to variations of considerable magnitude among the candles themselves.

However, in England the candle has been legalised by Act of Parliament, and although much abuse has been cast at the term "candle-power," it is, on the whole, a very suitable term, since to the unscientific it conveys the idea of a suitable magnitude which none of its proposed substitutes would do, while to the scientific observer the name is of little consequence, providing the quantity to which it is attached is capable of definite specification.

The candle, therefore, remains as our unit, and we must seek some means of reproducing its light equivalent by such means as will render it invariable under all conditions.

For this purpose there are two methods open to us, viz. we may raise a body to a definite degree of incandescence and measure the luminous radiations from a given area of its surface ; or we may burn some combustible in a burner of definite dimensions with a definite flame height.

Of these two methods the first would at first sight appear to recommend itself on the score of definiteness, since we are only required to provide an area and a definite temperature. But we must

first carefully enquire into the nature of the radiation given out by a body as its temperature is increased.

In 1847 Draper announced that if any solid body has its temperature continuously increased, it would become visibly hot at a definite temperature, and that the spectrum of the light so produced was a continuous one. This temperature, at which a red glow was just visible, was given by Draper as 525° C. As the temperature was raised, shorter wave-lengths were added. Thus a body at 720° C. emits all wave-lengths between the A ($\lambda = 7604$) and C (6562) lines, at 780° C. it extends from A ($\lambda = 7604$) to G (4307), and at 1165° C. it includes all wave-lengths from A ($\lambda = 7604$) to H ($\lambda = 3967$), and above this temperature the ultra-violet radiations are added.

Now H. F. Weber, in 1887, discovered that the first visible radiation from the heated body was not the red glow observed by Draper, but was a kind of indefinable grey which made its appearance in the spectroscope in a region corresponding to the yellow or greenish-yellow, and that the temperature at which this glow was observed was not the same for all bodies,[1] but having passed this point the brightness of the body increases very rapidly with the temperature.

It is due to the misunderstanding[2] of Kirchoff's law of 1858 that Draper's law was unopposed for so long. According to this fundamental law of Kirchoff, every body emits at any temperature wave-lengths which it itself absorbs at the same temperature. This law was generally understood to apply to gases instead of solids, and was generally treated from that point of view.

R. von Helmholtz, in 1889, pointed out that one of the reasons why Kirchoff's law held for solid bodies strictly was that since the absorption was so great in a solid, practically no rays could pass up from the interior to the surface, and we could therefore speak of the emission per unit surface. But in the case of gases, not only have we the surface emission but also an interior emission which can be varied with the pressure.

Hence Kirchoff's law may be expressed by saying that for every wave-length the ratio of the emitted energy to the absorbed energy is at a given temperature independent of the nature of the body when the light and heat emissions are due to heating, or to what R. von Helmholtz has called "regular" radiation.

It is understood from the above that the body absorbs all the energy which falls upon it, and does not transmit or reflect any. Such a body does not exist, but forms the ideal black body of Kirchoff, and its absorption co-efficient would be unity.

Now in lamp-black and spongy platinum, and many other black bodies, we have very fair approximations to the ideal black body; but unfortunately they are nearly all destroyed at very high temperatures. We may, however, produce an absolutely black body by other means, for, according to Kirchoff's law, the constitution of the radiation pervading an enclosure with perfectly opaque walls at a definite temperature, is a function of the temperature of those walls and of nothing else. Hence, if we imagine an enclosure—a hollow sphere, say—having a small aperture in it, and whose interior surface is coated with some black substance, if a ray enter obliquely at the aperture, it will be entirely absorbed in the interior, and we shall have realised our ideal black body. Any ray in the interior of this sphere will in quantity and intensity be equal to that coming from an absolutely black body at the same temperature.

Black bodies of this nature have been set up at the Reichsanstalt, consisting of carbon tubes heated to incandescence by an electric current, and protected by external jackets of carbon and other materials. And with such black bodies, Lummer and Wein have shown that all bodies radiate in exactly the same way in the interior of a hollow space kept at a uniform temperature.

From the above we conclude, therefore, that since a black body will radiate as much energy as it absorbs when equilibrium of temperature is established its radiant energy must be a maximum. The distribution of this energy in the spectrum of the black body, however, shows that most of this radiant

[1] Sitzungsber der Berliner Acad. (1887) p. 491.   [2] Engineering, vol. lxxvi. 1903.

energy is composed of wave-lengths too long to affect the eye, but there is a distinct shift towards the visible wave-lengths as the temperature is raised (see Fig. 1).

It is therefore obvious that, apart from experimental difficulties, the absolutely black body is not a good light radiator, and we must therefore seek a substance which will absorb and emit more luminous rays and transmit or reflect more heat-rays.

Furthermore, this source of luminous radiation should be white, and must therefore include the whole of the visible spectrum, and therefore, from what has been said above, it must be capable of withstanding a temperature of not less than 1165° C.

Since the body is to be a good reflector, we are limited practically to metallic substances, and of these only iron, nickel, gold, palladium, iridium, and platinum have melting-points high enough.

Of these, gold is but little above 1165°. Iron and nickel are not reliable at high temperatures, and iridium disintegrates. Platinum, on the other hand, seems pre-eminently suitable, since even in the liquid state it has a beautiful mirror-like surface. It is very unoxidisable, and can be procured in a high state of purity, and further, Lummer and Pringsheim have found that at red glow it radiates less than a tenth of the energy of an equivalent black body, and at the highest temperatures less than half (see Fig. 1).

M. Violle in 1881 first proposed to define the unit of light as the light emitted normally from 1 sq. cm. of molten platinum at its solidification temperature. He conducted researches with such a standard and demonstrated its superiority. In his researches[1] he used a large quantity of the metal (1 kg.). This was heated in a lime crucible by means of a blow-pipe burning oxygen and illuminating gas; when the metal was melted, the rays from the surface were either directly received on the photometer screen, or reflected on by means of a mirror. The temperature of solidification was indicated by the sudden increase of brightness or flash which always accompanies it, and which is easily appreciated on the photometer.

The congress of electricians at Geneva in 1896 adopted as the unit of luminous intensity the candle, defining this to be represented by the bougie-decimals of the previous Paris Congress of 1889, which defined the bougie-decimal as $\frac{1}{20}$th of a Violle unit.
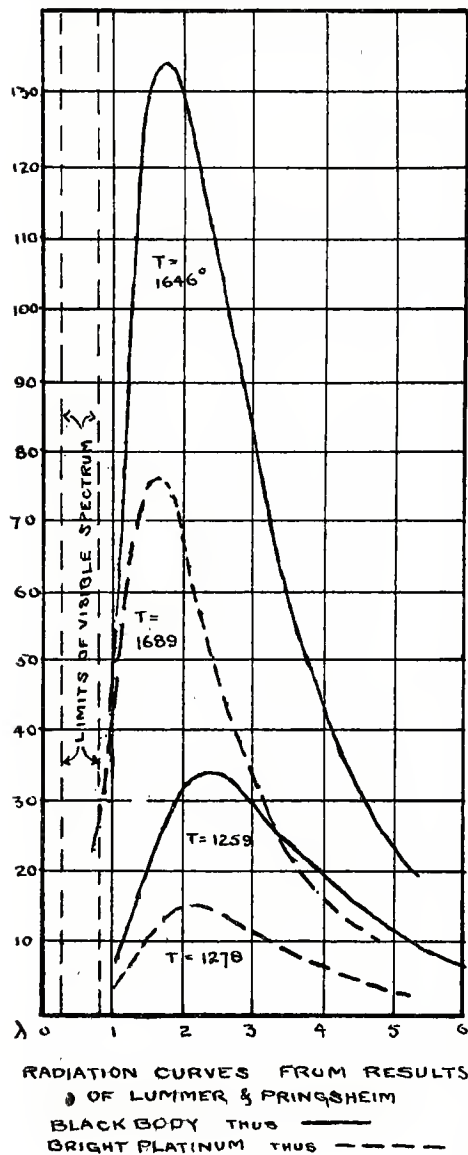
RADIATION CURVES FROM RESULTS OF LUMMER & PRINGSHEIM

BLACK BODY THUS ———
BRIGHT PLATINUM THUS — — —

Fig. 1.

From the very outset it was obvious that the platinum unit would not be commonly employed. But in adopting this unit the Conference cared more for the unification of photometric standards than for the creation of a light standard for photometric work.

Curiously enough, M. Violle in his researches makes no mention of any special difficulties in setting up his standard, and yet Lummer and Kurlbaum at the Reichsanstalt mention that it was tried and

[1] *Lum. Él*, vol. xiv. p. 475.

abandoned, owing to difficulties which arose from the impurity of metal and consequent change in surface, while Petavel in this country succeeded in reproducing it only by taking very special precautions.[1]

From his researches Petavel drew the following conclusions:—

    I. The ingot of metal should not be less than 500 grammes.

    II. The crucible should be of pure lime.

    III. The metal should be chemically pure.

    IV. The hydrogen must contain no hydro-carbons.

    V. The gases must be burnt in the proportion of 2 vols. of H to 3 vols. of O ; illuminating gas was found to be unsuitable by Condition IV.

If the above conditions are satisfied, it is probable that the variations of the light emitted is not greater than 1 per cent.

It must not, however, be overlooked, that the standard temperature is a transitory one, the period of constancy depending upon two things, viz.: the weight of the ingot employed, and the initial temperature to which it is raised, for Petavel has shown that unless a fair superheat be allowed, the curve does not suddenly rise to the constant temperature point, but gradually falls until it reaches it. The constant portion, which then merely forms an inflection in the curve, is somewhat difficult to determine (see Fig. 2).

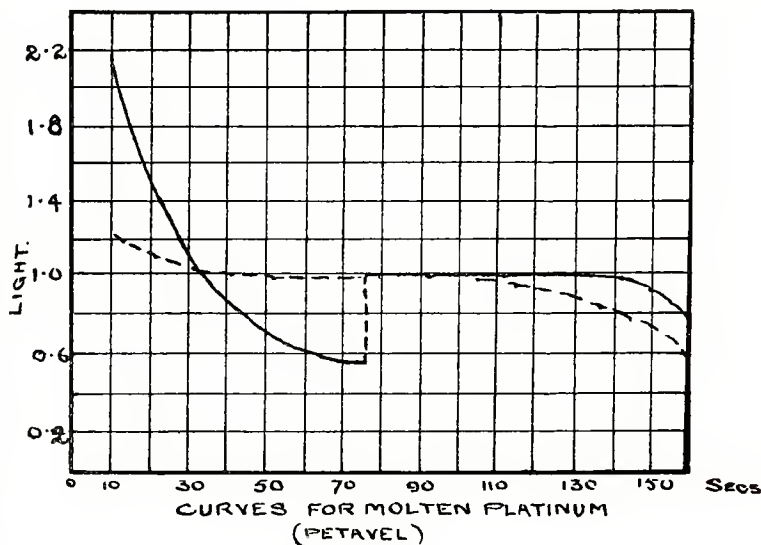
CURVES FOR MOLTEN PLATINUM (PETAVEL)

Fig. 2.

The duration of this constant period is, under the best condition, some 50 seconds, and there is therefore some difficulty in making measurements, on the wing as it were. Indeed Petavel used a self-registering photometer bench in his observations in order that no time should be lost in observing and recording.

The employment of oxy-hydrogen fusion with the necessarily elaborate apparatus connected with it has rendered the employment of the platinum standard difficult and troublesome, particularly since measurements must be made by a series of reheatings, as it is not possible to keep up a steady supply of heat.

The employment of electric fusion as a possible solution of this difficulty has been investigated but without much success by Petavel.

This lack of success is probably due to the fact that he endeavoured to regulate his heating by adjusting a current of some 2000 amperes on a mercury resistance. Now it is well known to electrical engineers that to control such a large current directly by a resistance is by no means easy, it being very difficult to hold it for any time to a steady value, small changes in the current making relatively large changes in the heating. Petavel found that it was not possible to fuse bars of more than 70 sq. mm. section with 2000 amps., and further, that even then they deformed their supporting trough of lime, became irregular in cross-section, superheated locally, and finally ruptured the circuit.

[1] J. E. Petavel, *Proc. Roy. Soc.*, vol. lxv. p. 649.

He then tried, with considerably more success, fusion in a crucible of nickel constructed in such a fashion that it could be kept cool; under these circumstances the platinum was melted in a crust of solid metal, but the quantity of fused metal was insufficient for working purposes.

Now Petavel suggested, but did not try, an alternate current transformer to supply his heating current, and the same solution independently occurred to Dr Drysdale and the author before we were aware of details of Petavel's work, and such a transformer has been designed, and we are hoping in the near future to construct it.

The following is a brief description of the instrument: The transformer is of the shell type, having a primary circuit designed to receive supply at 100 volts. 50 ω. This is inserted in the groove of the channel section secondary which consists of a single casting of copper. This disposal of the coils will therefore reduce magnetic leakage to a minimum. The platinum, which is to be raised to incandescence, will be cast between the horns of this secondary turn (which will be cooled by water supply), and will be contained in a crucible of pure lime or a nickel crucible constructed on the plan devised by Petavel. The floor of this crucible will be so shaped that the heating will be localised to the central portions of the strip, in fact, we hope to form a little lake of molten metal surrounded by walls of the solid metal itself.

From the dimensions I have calculated that about 140 grams. of platinum will be required, and the capacity of the transformer will have to be about 6 K.W., and consequently the primary current at 100 volts. will be 60 amps., a current capable of easy and exact manipulation.

Fig. 3 is reproduced from the constructional drawings, and shows the apparatus to be compact and simple, and it should be possible to control the heating with a great degree of precision, and hence prolong the period of solidification considerably, and at the same time use only about one-fourth the quantity of platinum specified as necessary by Petavel.

It will be gathered from the foregoing that the production of a standard of light by such simple means as raising a solid to a definite degree of incandescence is beset with such difficulties as to render it impractical as a working standard, although both Siemens and Liebenthal have endeavoured by fusing a platinum ribbon with an electric current to produce a secondary standard on this principle.

It has been proposed independently by Mr Swinburne,[1] Professor S. P. Thompson,[1] and M. Blondel to use the radiation from one square millimeter of crater of a direct current silent arc between carbon electrodes as the standard of light. M. Blondel and Mr Petavel have both conducted careful measurements of this quantity.

The choice of such a unit is based upon the hypothesis that the crater of the arc is at a constant temperature, that of volatilisation of carbon, for it has been observed that the area of crater increases in proportion to the current, and that the light increases in the same ratio as the area of the crater.

Further, Sir W. Abney has shown that the light emitted from the crater has always the same spectral composition, and hence the temperature of this light-emitting surface must be constant. This, in fact, ought to be the case if the latent heat of vaporisation of carbon is a positive quantity, for when this temperature is reached and volatilisation has commenced the temperature cannot rise any further.

M. Blondel's measurements[2] were conducted with a projector lamp, an image of the crater being formed by means of a pin-hole diaphragm placed close to the arc and kept cool by water circulation. An inverted image of the arc was thus formed upon a screen which contained an aperture of definite dimensions arranged to fall well within the luminous area, and the radiation from this aperture is then measured.

The little hole in the cold diaphragm was liable to become blocked with carbon dust, and to obviate this difficulty, Blondel proposed to use a lens of known surface and co-efficient of absorption to form the image. By this means the lens could be placed some distance from the arc and the water cell omitted.

[1] *Jour. I.E.E.*, vol. xxi. 1892   [2] A. Blondel, *Elect.*, vol. xxxii. p. 117.

From his experiments Blondel drew the following conclusions :—

    I. The average brilliancy of the incandescent portions of the arc, taken as a whole, increase with the current and also with the current density until the crater is saturated.

    II. If the current is suddenly altered the intensity drops until the crater adjusts itself to saturation.

    III. The carbons must only contain small amounts of foreign substances.   Most carbons were found to contain not more than four per cent. of impurity, two per cent. of which were mineral salts.

The intrinsic brilliancy is given by Blondel as 160 bougie decimals per sq. mm.

The above researches were repeated by Petavel[1] in this country in a very thorough manner, using an arc with carbons at right angles, as well as several other forms.

The objects of his researches were as follows :—

    I. To determine the average brilliancy of a normal (silent) arc.

    II. To determine, when conditions are carefully specified, if variations are still too great to allow the use of this source of light as a standard.

    III. What effects are produced by use of excessive currents and current densities, and by surrounding the arc by an enclosure maintained at a high temperature.

The results of these enquiries he sums up as follows :—

    I. The intrinsic brilliancy of the crater of a silent arc is about 147 cp. per sq. mm.

    II. Even under the most favourable conditions it is difficult to obtain consistent results, variations of five per cent. not being infrequent.

    III. Variations in size of carbons, intensity and density of current, in length of arc, and total power expended as long as the arc is silent, do not cause the intrinsic brilliancy to vary more than ten per cent. on either side of the mean.

    IV. The enclosure of the arc in a chamber maintained at 900° C. does not cause any sensible variation in the intrinsic brilliancy.

From the above conclusions it will be seen that, although experiments confirm the theory that the crater of a carbon arc is at the temperature of the volatilisation of carbon, owing to the difficulty in obtaining consistent results, its employment as a standard is at present not advisable.   I should, however, like to raise one point of criticism here.   M. Blondel gives as the value of the intrinsic brilliancy of 1 sq. mm. of the arc crater—160 bougie-decimal.

Now a bougie-decimal is $\frac{1}{20}$th of a Violle unit, and Violle gives the value of 18·5 English candles to the platinum unit.   Hence on this basis M. Blondel's value for the arc in English candles is 148 cp.

Now Petavel has quoted 19 cp. for the platinum unit,[2] and even upon this basis M. Blondel's value is only 152 cp.   The highest value of the bougie-decimal I can find recorded is ·984 British candles (a none too reliable figure), and only by using this does M. Blondel's figure reach 157·4 cp. The mean value of the bougie-decimal from M. Violle's and Mr Petavel's experiments (which are the only direct determinations) put M. Blondel's value for the arc at 150 cp.

Now it will be found that Mr Petavel has quoted M. Blondel's value as 158 cp.,[3] and this has been several times copied by various writers on this subject without comment.

It is interesting to note that M. Blondel quotes in his paper[4] the figure 158 *bougie-decimals* as a value obtained by him, but in his final conclusions he prefers the higher figures, all of which are given in *bougie-decimals*.

If the above reasoning is correct, then the only two direct determinations of the light emitted from the arc crater are in much closer agreement than is usually supposed, and the value of the light as a standard is considerably enhanced.

In consequence of the difficulties experienced in obtaining consistent results with the Violle

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[1] *Proc. Roy. Soc.*, vol. lxv.
[2] *Elect.*, vol. l., 1903.
[3] *Elect.*, vol. xliv. p. 712.   1900.
[4] *Elect.*, vol. xxxii.

standard at the Reichsanstalt, Drs Lummer and Kurlbaum were led to adopt another method of forming a light standard.

In their arrangement a strip of platinum foil of standard dimensions was raised to incandescence

PRIMARY
COIL

PROPOSED ELECTRICALLY HEATED
PLATINUM STANDARD

INCHES

Fig. 3.

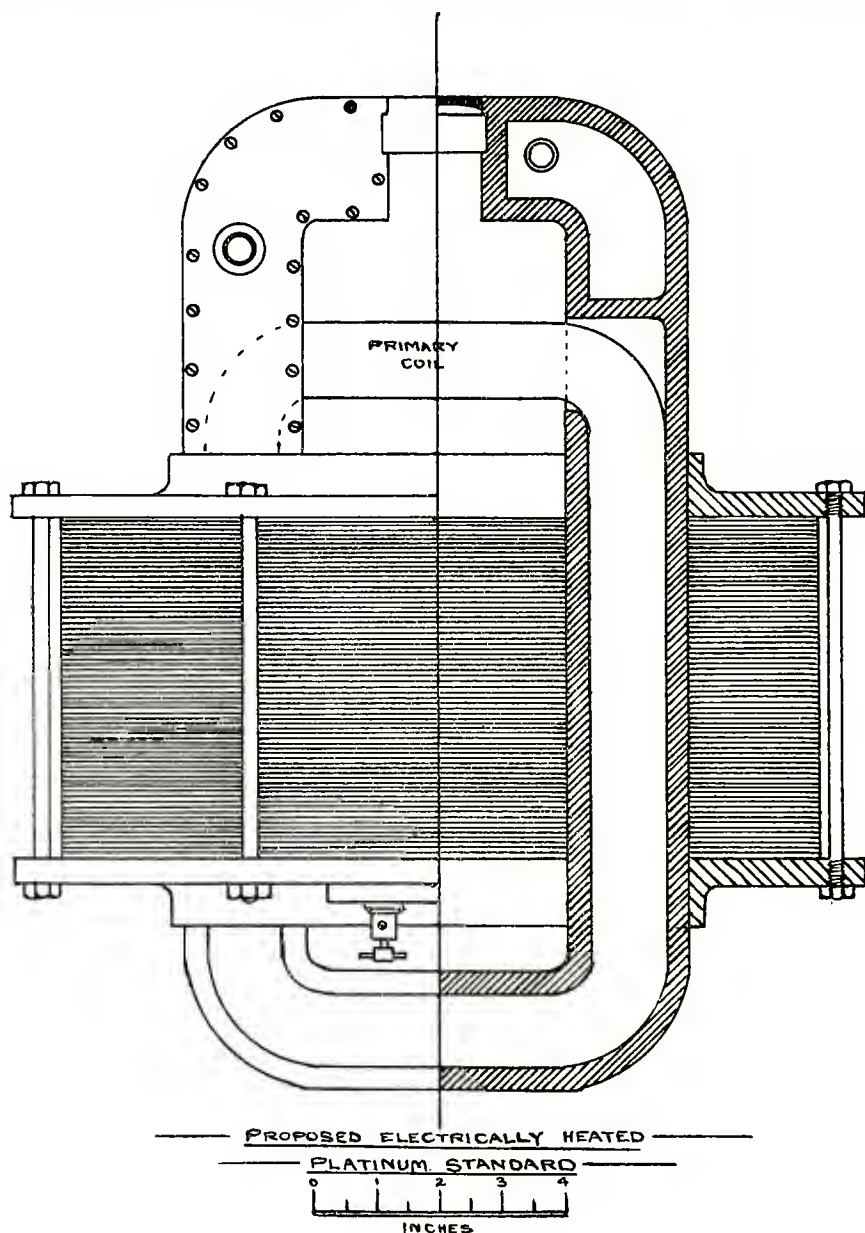by means of an electric current, and the temperature was adjusted until $\frac{1}{10}$th of the total radiation was transmitted through a thickness of 2 cms. of water contained in a trough having quartz sides.

This ratio of radiation was measured by means of a bolometer, an instrument consisting of a delicately-constructed platinum grid. The radiation falling upon this grid alters its resistance, and

if therefore it be included in one of the arms of a Wheatstone bridge, there will be a disturbance of the bridge balance, due to the changes in resistance, the amount of which will be proportional to the radiation falling on the bolometer.

Petavel during his researches upon primary standards of light investigated the possibilities of this standard, but did not come to very favourable conclusions. The electrical measurements required to be made with great precision, whilst the bolometer itself was difficult to adjust to constancy when measuring the ratio of the radiations by different methods. A change of bolometer made considerable alteration to the value of the standard, whilst the adjustment of the current (about 80 amps.) was, except under the most favourable conditions, tedious if not impossible. It is possible, however, that this adjustment might have been simplified considerably by the use of a transformer. Finally, the light emitted was too red for general working, although this defect is not so important where (as in the Reichsanstalt) the standard is set up for standardising Hefner lamps.

As an outcome of his unique and valuable work upon primary standards of light, Mr Petavel [1] came to the conclusion that only three substances are available as radiators, viz. platinum, platinum iridium (25 per cent. iridium), and iridium. He gives the intrinsic brilliancy in candle-power per sq. cm. as 19, 34, and 290 respectively.

The intensity of the light emitted from iridium near its melting-point is remarkable, being some fifteen times that of platinum, but unfortunately the foil disintegrates at this high temperature if the heating is continuously maintained.

The next important point is that of temperature regulation, and here he finds it best to determine the temperature of the radiator by means of the relative intensity of the radiation of two different wave-lengths.
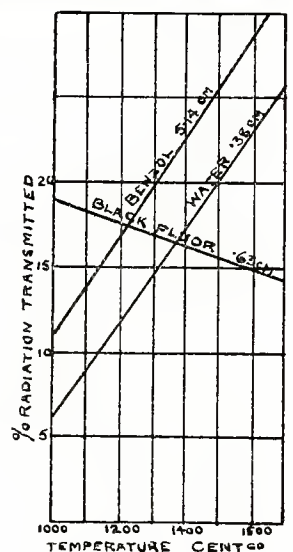
For metallic radiators, as for a black body, the wave-length at which maximum radiation occurs is in inverse proportion to the absolute temperature.

Two wave-lengths are therefore selected, one in the infra red and one in the visible spectrum. If we measure in each case the energy of radiation in percentage of the total we shall find that as the temperature rises the former decreases and the latter increases, and at one temperature only will they be equal.

ABSORBTION CURVES
(PETAVEL.)

Fig. 4.

For purposes of analysing the radiation, the difference in thermal absorption of various bodies was investigated. For most substances it was found that the percentage of heat transmitted increased with the temperature of the radiator, with one exception, black fluorspar. Now since water and glass are practically opaque to radiations of greater wave-length than $3\,\mu$, and black fluorspar is entirely opaque to the visible spectrum, these substances may be used for analysing the radiations from the radiator. To secure accuracy, the rate of variation with temperature should be large, and water, benzole, and glass all fulfil this condition (Fig. 4).

The standard is obtained by dividing the light from the radiator into three beams. One, passing through a window of black fluorspar, falls upon the face of a thermopile. The second, passing through a window of water or benzole, falls upon a second thermopile. These two piles are connected in opposition through a sensitive galvanometer, and when this instrument reads zero the temperature is right, and the central beam is then of the right intensity and can be used for photometric comparisons.

Although this method has some advantage over the Lummer-Kurlbaum standard from the simplicity

[1] *Elect.*, vol. 1, p. 1012. 1903.

of its electrical adjustments, the use of thermopiles and their attendant troubles is an objection, since they are very liable at times to give uncertain indications of small differences of temperature.

Schwendler has proposed to use a horseshoe-shaped platinum strip mounted in an exhausted bulb and raised to incandescence by an electric current, but the suggestion has never been largely adopted owing to variations in the emissive power of the surface of the strip, alteration in cross-section, and other troubles of a like nature.

Sir W. Preece and later Edison in America have proposed the use of carbon filament lamps, but the objections to this standard may be summed up as follows :—

The light-giving power of the filament vary from the following main causes :—

     (I.) Changes in Resistance of the Filament.
     (II.) Changes in the Surface of the Filament.
     (III.) Projection of Carbon on to the interior of the containing bulb.

The convenience of a glow lamp secondary standard cannot, however, be disputed since it is independent of all external atmospheric changes (with perhaps the exception of temperature), and this has led Prof. J. A. Fleming to devise his standard form of lamp.

According to his experience, it is possible to bring a good carbon filament into a constant condition by running it for some fifty hours at slightly more than its rated voltage. The filament so aged is taken from its bulb and mounted in a specially large clear bulb, some 10 cms. in diameter and 20 cms long ; under these circumstances the first two defects are met by the ageing, and the last is prevented or considerably delayed by mounting in the large bulb.

Such lamps if only run for short periods form valuable secondary standards. But, owing to the large negative temperature co-efficient of carbon, a comparatively small alteration of voltage causes considerable alteration in candle-power. Hence, if the standard is to be kept right to 1 per cent., the power it absorbs must be adjusted to 0·4 per cent. This therefore necessitates the use of some very delicate potential measuring device such as a potentiometer, and, further, it is practically imperative that the power should be supplied from secondary cells. Unless these precautions are taken the results will be of very doubtful value.

All the foregoing standards which depend for their light-giving power upon the incandescence of a solid substance may be used as ultimate standards of reference (with perhaps the exception of the Fleming lamp), and are designed to be constant and free from periodic and permanent changes, and to be reproducible from dimensions and specification. They are, however, not suitable (again excepting the Fleming lamp) as working standards, on the score that in most cases they require the employment of delicate and complicated apparatus which is by no means portable.

On the other hand, if we employ the alternative method of producing light by burning some combustible material in air or in an atmosphere of oxygen, we have an essentially portable light.

Numerous researches have been made to secure a suitable flame standard, with the result that it is invariably found that to secure only approximate uniformity we must have a fuel of definite chemical composition, and that this must be consumed under constant conditions.

Of the numerous gas, oil, and candle flames, none absolutely comply with these conditions, the last of which is the most difficult to fulfil. Yet an enormous amount of expert attention has been paid to flame standards, which would perhaps have been more profitable if directed to the more scientific incandescent standards, for once a perfect incandescent standard is established, the study of the behaviour of flames becomes an easy matter, and unification of standards become possible.

At the very outset we have to face the difficulty that the light-giving properties of all flames are affected by the condition of the atmosphere, and consequently, if accurate results are to be obtained, we must determine the pressure, humidity, and percentage of CO_2 present in the air at the time of the measurements. These corrections are ignored by gas engineers on the assumption that the flames they test are affected in the same manner as their standard ; but with electric lamps which are not so affected by atmospheric conditions these corrections become appreciable, and three auxiliary determinations
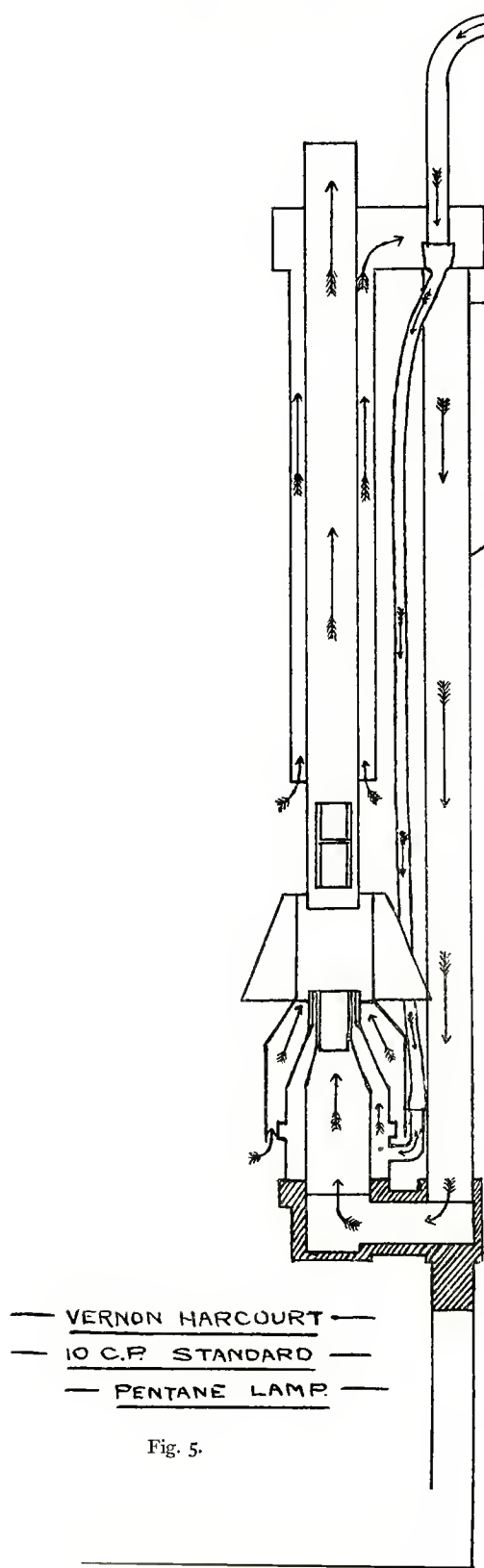
VERNON HARCOURT
10 C.P. STANDARD
PENTANE LAMP.

Fig. 5.

become necessary. The uncertainty of the primitive flame standard, the candle, led to the introduction of lamps consuming oils and combustible organic compounds rich in carbon, and an endeavour to produce standard gas flames.

There are three conditions that the ideal flame standard should fulfil, viz. :—

I. The fuel should have a definite chemical composition, and its purity should be capable of easy verification.

II. It must be consumed under easily controlled and well-defined conditions in a burner of definite dimensions.

III. Atmospheric changes should not affect the quality of the flame.

In this country the lamp most nearly fulfilling the above requirements is probably that devised and introduced by Mr A. G. Vernon Harcourt in 1877. This lamp, which has passed through several modifications since its introduction, employs as its standard fuel, pentane.

This is the distillate[1] yielded by light American petroleum after three fractional distillations, and subsequent treatment with sulphuric acid and caustic soda. The product has the chemical composition C_5H_{12}, and forms a mobile volatile liquid, giving off a highly inflammable vapour.

The density of the liquid should not be less than ·6235 or greater than ·626 at 15° C.

The density of gaseous pentane should lie between 36 and 38.

Admixtures of hydro-carbons belonging to other groups such as benzene or amylene must be avoided.

Their presence may be detected by the following test : 10 cc. of nitric acid of 1·32 sp. gr. (made by diluting pure nitric acid with half its bulk of water) is introduced into a white glass-stoppered bottle.

[1] Gas Referees' Notification of 1901.

Add 1 cc. of a solution of potassium permanganate containing ·1 gram of permanganate in 200 cc. Add to these 50 cc. of the sample of pentane, and shake vigorously for five successive intervals of twenty seconds; if no hydro-carbons other than paraffins be present, the pink colour will still be distinct. If any other hydro-carbons be present the pink colour will be discharged.

The purity of the pentane is a very important point in these lamps, if the standard light is to be reproduced.

Two forms of this lamp are in general use, viz. the portable 1-candle industrial form, sometimes called the Woodhouse & Rawson pattern, after the firm that made it in 1890 and the most recent form of chimneyless 10-candle standard (Fig. 5).

In the former model the pentane is raised to the burner by means of a circular wick. This wick tube is surrounded by an outer tube of larger diameter, so constructed that a current of air passes up round the flame from below. Over the top of this outer tube a metallic chimney is supported, the distance between the top of the outer tube and the bottom of the upper chimney being accurately set by means of a standard cylindrical steel gauge. Thus all but a definite length of the flame is cut off by the chimney and outer tube. As it is important to regulate the flame height, two narrow slits are cut in the upper chimney, and the tip of the flame is regulated to be in the centre of these. It was with this type of lamp that Liebenthal made his investigations as to the effects of barometric pressure and humidity. He expressed his results in the following equations :—

For variation of pressure—

$$\Delta L = ·00049 \, (H - 760).$$

Where ΔL is the variation in light corresponding to a barometric height of H millimetres.

For water vapour

$$L = 1·232 \, (1 - ·0055w).$$

Where w = the number of litres of water vapour per cubic metre of air, the formula holding good for 4 to 18 litres.

With regard to flame height Mr Harcourt[1] gives the following rule for correcting the height of the flame. The standard height of flame for 1 candle is 63·5 mm. at 30 inches barometric pressure. For every tenth of an inch above or below 30 inches, the flame must be set an equal number of fifths of a millimetre below or above 63·5 mm.

Further, Professor Ayrton[2] has pointed out that considerable discrepancies occur in the dimensions of different examples of this lamp, and these differences will account for errors as large as 12·5 per cent. between measurements made with two standards. He further states that much depends upon the way the wick is treated, and the author's investigations on one of these lamps fully confirm this point, the charring at the top of the wick accounting for about 2 per cent. variation in the light.

Another defect of the lamp is the heat-capacity of the metal parts—the lamp taking from a half to three-quarters of an hour to reach a steady state. I have described the performance of this type of lamp at some length, as it is even now largely used. In the more recent types of Harcourt lamp the wick is dispensed with, and the pentane vapour from the liquid pentane contained in the upper reservoir (Fig. 5) descends by means of the flexible tube to the Argand burner at the base, over which is a double metallic chimney. The air supplied to the centre of the burner is drawn up between these two chimneys, passes down the hollow pillar of the lamp, and thence to the centre of the burner. The chimney is adjusted so that its lower edge is 47 mm. from the steatite ring of the burner, a boxwood gauge being provided for setting the distance. A window is provided in the chimney to adjust the height of the flame, which must be done with some exactitude.

The effects of variations in barometric pressure and humidity on this type of lamp have been investigated by Mr C. C. Paterson at the National Physical Laboratory, who has given the following formula :—

$$Cp = 10 + 0·66(10 - \epsilon) - ·008(760 - b),$$

where ϵ is the humidity in litres per cubic metre, and b is the barometric height in mm.
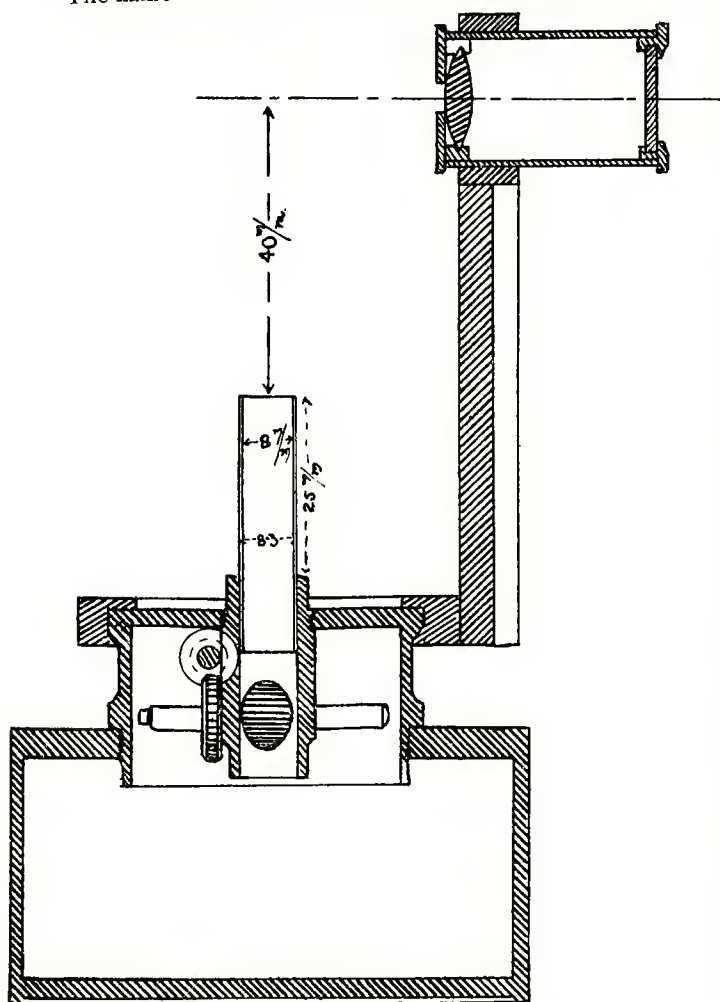
[1] Harcourt, Brit. Assoc. Aberdeen, 1885.　　[2] *Jour. Elect. Engineers*, vol. xxxii., pp. 200.

These pentane lamps have not only proved valuable on account of their constancy, but also for the excellent white light they give. The flame is, however, very lambent, and therefore much affected by draughts.

The author has, so far, been unable to obtain figures connecting the variation of the light of these standards with the amount of CO_2 present in the air, and from experience with the lamps it would seem that this is of considerable importance.

The flame standard on the Continent and in America is the Hefner lamp (Fig. 6). This lamp, which is a simple style of spirit-lamp, burning acetate of amyl, is an improvement on the early benzine lamps, among the first of which was the lamp of Eitner in 1877, which consumed benzine.

With a view to eliminating errors due to the indefiniteness of the chemical composition and the unequal rates of combustion of the constituents of the fuels used, Hefner von Altenack investigated the effect of burning different liquids in these simple spirit-lamps, and came to the conclusion that amyl acetate was the best substance to use. This fuel, which has an intense, agreeable odour, has the chemical composition $CH_3 \cdot COOC_5H_{11}$. It is prepared from amyl alcohol by distillation, with crystallisable acetic acid and sulphuric acid; its boiling-point is 138° C. It is burnt in a lamp of the simplest construction, the burner consisting of a German silver tube 8 mm. internal diameter, ·15 mm. thick, and 25 mm. high. The fuel is raised to the top of this burner by a wick of cotton threads, just filling the tube without crowding. This wick should be maintained level with the top of the burner tube, and if the reservoir is kept plentifully supplied with fuel, the surface of this wick should not char. The flame is tapering, and

HEFNER AMYL ACETATE . LAMP.

Fig. 6.

burns without chimney, and should be regulated to a height of 40 mm. from top of burner to top of flame.

The excellent work of the Reichsanstalt, the German Gas Commission, and Liebenthal have considerably enhanced the value of the lamp by determining its constants to a great degree of precision. And as a result of these investigations, when lamps are constructed to strictly prescribed dimensions and materials, the illuminating power is reducible to within 2 per cent.[1]

[1] Prof. E. L. Nicol's *International Congress of St Louis.* 1904.

But C. H. Sharp,[1] when comparing two Hefner lamps certified by the Reichsanstalt, found their mean difference was 1·2 per cent., although on two occasions this difference between the lamps amounted to 5·2 and 3·9 per cent.

The effects of flame height, water vapour, carbon dioxide, and pressure have been investigated by Liebenthal, and are expressed by the following formulæ :—

For water vapour :

$$L = 1·049(1 - ·0053w),$$

where w are the litres of water vapour per cubic metre.

For carbon dioxide :

$$L = 1·012(1 - ·0071c),$$

where c is the quantity of CO_2 in litres per cubic metre (Fig. 7).

CURVES SHOWING INFLUENCE OF CO_2 & MOISTURE ON FLAME OF HEFNER LAMP.
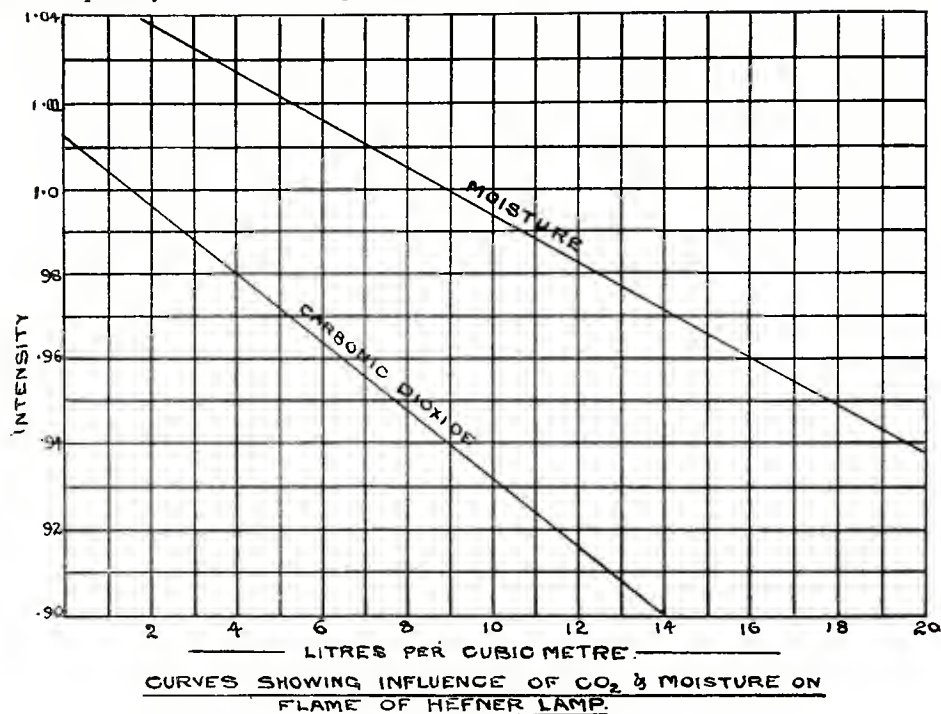
Fig. 7.

For variations in barometric pressure :

$$\Delta L = ·00011(H - 760).$$

And for flame height :

$$L = (1 + ·025(h - 40)); \text{ or}$$
$$L = (1 + ·030(40 - h)),$$

where h is the actual height of the flame above or below 40 mm. (Fig. 8).

From the above it will be seen that the light of the Hefner lamp decreases ·5 per cent. per litre of water vapour, and is correct at 8·8 litres per cubic metre.

It also decreases about ·7 per cent. per litre of CO_2, being unity at 1·8 litres per cubic metre.

A change of 1 mm. in the height of the flame causes 3 per cent. change in the light, and a change of 10 mm. pressure produces ·1 per cent.

Unless these corrections are applied to the lamp, particularly that of CO_2 and water vapour, the results obtained with it will be considerably in error.

[1] *Trans. Am. I.E.E.*, 19, pp. 1493. Wor. 1902.

In modern lamps the Krüss optical flame gauge is usually fitted. This consists of a lens mounted at one end of a short tube which forms an image of the tip of the flame on a ground glass screen at the other end of this tube ; across this screen a line is drawn corresponding to the height, 40 mm. ; with this device the flame height can be adjusted with considerable exactitude.

Despite the facts that the constants of this flame are so well known, and that the lamp is easy to manipulate, is extremely compact and not liable to get out of order, it has found but little favour in England. It is urged that the light is too red for ordinary work, and the unprotected flame is subject to draughts.

Notwithstanding this, it may be justly claimed that the lamp forms one of the most useful and accurate flame standards in use at present.

The somewhat large correction for CO_2 needs only applying when this impurity is present in very large or very small quantities, since the lamp gives the correct light when 1·66 litres of CO_2 are present, and this corresponds to a very unhealthy state of the atmosphere, while, for absolutely pure air, the light is only 1·2 per cent. higher (see Fig. 7).

The light value of the lamp or Hefner unit is less than one English candle. The exact ratio of the Hefner unit to the candle is somewhat in dispute. Various determinations vary from ·872 to ·98, the most generally accepted value being ·88.

In 1902 F. v. Hefner Alteneck published an account of the various attempts to improve the Hefner lamp which had been made since its introduction in 1884. The German silver used for the wick tube is liable to be of somewhat indefinite composition, but it was found that this did not materially affect the light, and no change is therefore made. Again, the amyl-acetate is not a pure chemical substance, and its extremely penetrating odour has proved an objection. L. Knorr has investigated possible alternatives,

CURVE SHOWING INFLUENCE OF FLAME HEIGHT OF HEFNER LAMP

Fig. 8.

and the best appear to be acetylacetone, acetacetic ether, and isobutylacetate. The two former, however, are acid, and attack the metal parts of the lamp. Various mixtures of benzole and alcohol have been tried. It was found that these mixtures burned at a fairly uniform rate, and the light was more yellow, but, on the whole, the mixtures have not any great advantage. The effect of impurities have been investigated with great care by Liebenthal, who has found that they diminish the illuminating power to a slight extent. Amyl alcohol may be present in considerable quantity without harmful results, 20 per cent. only lessening the light by 1·1 per cent. If the amyl-acetate is saturated with water, a diminution of only ·5 per cent. is observable. Alcohol (methyl) seems to have the greatest effect. From the above it will be seen that the purity of the amyl-acetate is not of very great consequence if the light-giving power is considered, but impurities have a very marked effect in producing unsteadiness of the flame.

The following method, due to Bannow, is suggested for verifying the purity of the fuel.

The density of the liquid should lie between ·872 and ·876.

The mixture of equal volumes of amyl-acetate and benzine should remain clear and liquid if amyl alcohol or ethyl hydrate are absent, or if carbon disulphide be substituted for the benzole, water will separate out in globules.

A mixture of 1 cc. of amyl-acetate with 10 cc. of 90 per cent. alcohol and 10 cc. of water, a clear liquid solution is obtained if toluene, etc., is absent.
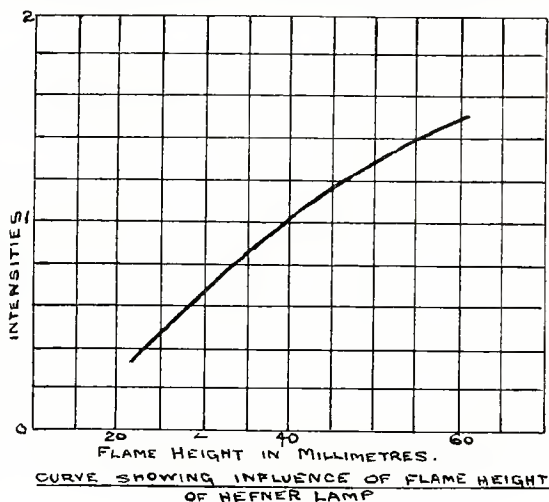
A drop of amyl-acetate should leave no greasy trace on white paper after evaporation if oils, tar, and other greasy materials are absent.

M. Blondel[1] has constructed a modified Hefner lamp, making use of a chimney to prevent air currents affecting the flame, the fuel used in the lamp being either amyl-acetate or crystallisable benzole and alcohol.

The French Standard Carcel lamp has found but little favour among photometricians anywhere but in France, principally owing to the fact that the fuel which it consumes, purified rape-seed oil, is not of very definite composition. On account of the viscosity of the oil it has to be raised by mechanical means to the top of the wick which, unless the supply is copious, is liable to charring, with consequent alteration of the light. The nature of the wick also influences the light as does also the rate of consumption of the oil; this should not vary beyond the limits of 38 to 46 grams per hour, 42 grams being the correct consumption. It is curious, however, that such experts as Fresnel, Dumas, Regnault, and Crova have used this standard in their researches and spoken highly of its qualities.

Of suggestions to use illuminating gas as a photometric standard, probably those of John Methven and Giroud are the most important.

Methven found that if ordinary illuminating gas be enriched with gasoiene or pentane, and burned in an Argand burner, a very constant light was produced, and by using a screen in front of the flame with an aperture of definite dimensions a very easily manipulated intermediate standard could be produced.

Giroud aimed at producing a candle burner supplied by an automatically controlled supply of gas. But although both these suggestions are excellent on the score of convenience they must even for industrial use be referred to some superior standard before their values can be ascertained with any exactitude, and therefore only form sources of light which may be relied on to remain constant over fairly long periods.

In this connection few observers seem to realise that the ordinary petroleum lamp supplied with a round burner like the Carcel lamp and a constricted glass chimney forms a very constant source of light, and is quite equal to the Methven standard if used in the same way with a screen and aperture. The best kerosene as supplied in commerce differs but very little in its general properties, and if this fuel be burnt in such a lamp a very handy and easily managed source of light is produced. Weidemann in 1881 and Hefner Alteneck have both spoken highly of such lamps, and the author has found only very small variations in its light (less than 1 per cent.) when burning for periods of several hours after the lamp is thoroughly warm.

The use of acetylene as a standard flame has for a considerable time occupied the attention of photometricians. The definiteness of its chemical composition, the ease with which it can be made, and the intense whiteness of its flame are all points strongly in its favour. But unfortunately no entirely satisfactory burner which will give an absolutely reproducible flame has been devised up to the present.

M. Violle has been working at this problem since 1895, when he proposed to use a flame and screen of the Methven type. And many—notably Nicols, Sharp, Hartmann, and Fery—have also done a considerable amount of work on the subject.

Owing to the richness of the gas in carbon it is necessary to burn it diluted with a certain amount of air, and if it were only possible to burn the proper mixture in a burner of definite dimensions it would probably form a most suitable standard; but on account of the highly explosive nature of the mixture the orifice of the burner has to be so small that accuracy of workmanship cannot be guaranteed, and further the orifice itself is subject to clogging with graphitic carbon, and consequently a change in the proportions of air and acetylene.

C. H. Sharp has burned acetylene in a mantle of oxygen, using a concentric burner, the acetylene passing out through the central orifice, and the supply of oxygen from the annular space between the tubes. This flame is very intense, but so sensitive to pressure changes as to render it unserviceable.

[1] *Ecl. Electr.* 16, p. 317. 1898.

Hartmann has studied mixtures of acetylene and hydrogen burning in air, and has shown that when the mixture is of equal volumes the intensity is a maximum, and the flame is whiter than pure acetylene burning in air. Owing to the difference in the solubility of hydrogen and acetylene, it is not possible to store the mixture over water, but the gases may be supplied separately to the burner, providing proper precautions are taken to ensure an equal and even mixture.

Summarising the foregoing review of light standards, we may draw the following conclusions :—

    I. That the ultimate standard of light will in all probability be a body in a definite state of incandescence.

    II. That platinum seems to be the most suitable substance for the purpose.

    III. That it will be preferable to use this substance at its point of solidification, providing this point can be maintained for a sufficient period, as the determination of any intermediate temperature involves the use of delicate and complicated auxiliary apparatus.

    IV. Flame standards are unsuitable as ultimate standards on account of atmospheric changes and difficulty in burning the fuel under constant conditions.

    V. That of flame standards the modern forms of Pentane lamps and the Hefner lamp are the best and most reliable, particularly the latter form of lamp, since its constants have been so carefully determined

Photometers.—From the production of a light standard we now turn to the methods of measuring intensity. Now this can only be accomplished by observing some phenomenon produced by the light itself. Since photometrically the only wave-lengths we are concerned with are those which lie between ·35 and ·8 μ,—that is, the limits of the visible spectrum—and the intensity of the light expresses the sensation of brightness produced in the eye itself, our photometric measurements must ultimately be resolved into some form of visual measurement.

Hence photometers, which are produced with a view to eliminating the eye and its vagaries, do not measure the intensity of the light at all unless they are first calibrated by some eye method, and even then their indications may prove fallacious.

I might, for instance, calculate the intensity of a beam of monochromatic light from a knowledge of the energy of vibration of the ether, providing I had some method of measuring the amplitude of vibration, but it is extremely doubtful whether the result so obtained and the visual result would be in agreement.

The ultimate measuring instrument, therefore, is the eye, and the principle upon which all photometers is based is to present to the eye a surface or two identical surfaces upon which the light to be measured and the light from a standard source are falling, and to modify the standard beam until the eye pronounces the illuminations equal.

Hence the photometric process consists of two essentials :—

1. The production of two identical surfaces or one surface, illuminated separately from each source.

2. The modification of the light from the standard source, until equality is reached.

As long as the lights are of the same tint neither of these essentials is difficult to attain, and most photometers assume this in the first place.

It would be impossible to detail all the various forms of photometers which have been evolved, and I therefore propose to confine myself more to the principles involved, and describe only the more typical and modern. The earliest forms due to Bouguer and Foucault employed a translucent screen illuminated by the sources from behind, and having a thin dividing partition, the brightness of the two halves being adjusted. Lambert or Rumford illuminates an opaque white screen with both sources of light, and interposes an opaque rod into the crossed beams in such a way that each throws a shadow on to the white surface. Equality of the density of these shadows is then a measure of equality of intensity.

This type of photometer has been exclusively used and advocated by Sir W. Abney in all his work upon colour measurement.

But the author, in common with a great many other observers, has not found it of very great service, principally on account of its insensitiveness. Considerable difficulty is experienced in judging when the shadows are equally grey, and large personal errors are also possible. One of the best methods of using the instrument is that suggested by Sir W. Abney, viz., to arrange the rod so that the two shadows just touch. Cover the screen with a black mask having a rectangular aperture in it over the position of the shadows; the indications of the instrument are then much more exact, since the eye is not troubled with the light from the surrounding brightly illuminated surface; but even under these

LUMMER BRODHUN
PHOTOMETER

Fig. 9.
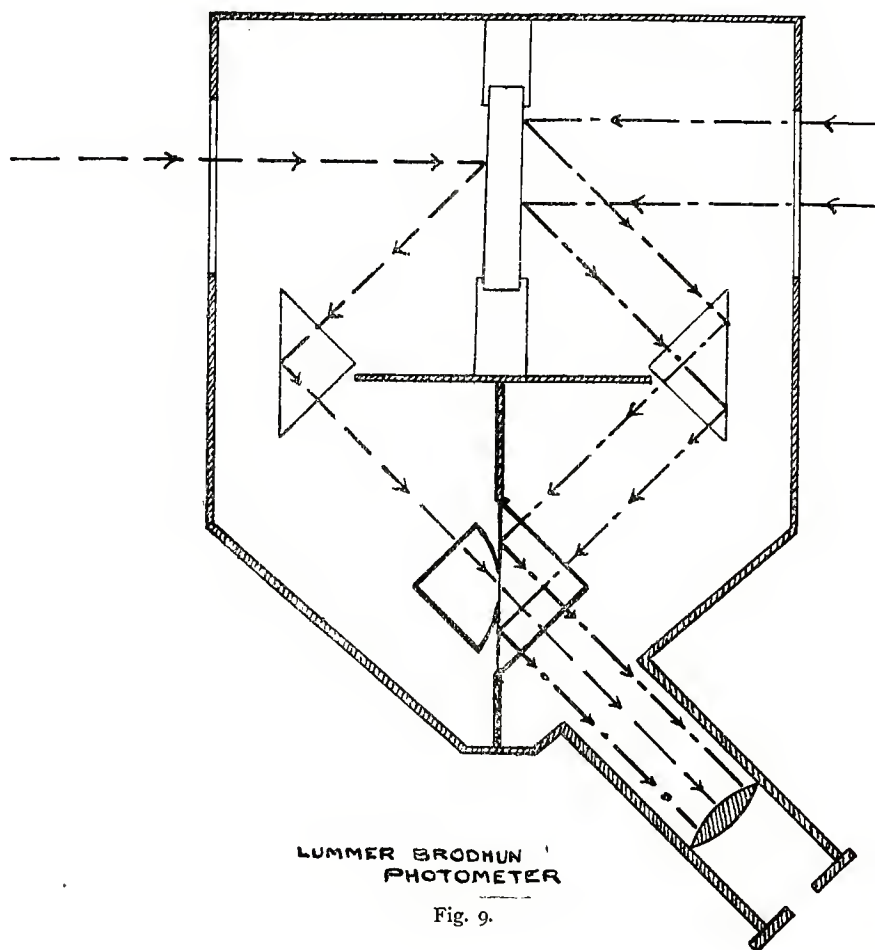
circumstances, the photometer, on the score of sensitiveness, is very inferior to most modern ones. Its one advantage is, that stray light is of little or no consequence.

In all the foregoing types the sensitiveness is greatly increased by making the dividing line between the two surfaces as fine as possible. This feature is beautifully achieved in the photometer used by the gas referees and known as the photoped. In principle it is really the inverse of the shadow photometer.

A tube is closed at one end by a plate in which a rectangular opening is cut. Inside this tube a translucent screen is mounted in such a manner that its distance from the opening may be adjusted. If now two sources of light be set so that they make equal angles with the axis of this tube they will each throw a patch of light on to the translucent screen; and if the screen be moved until the inner edges of

the two patches touch, the dividing line may be made of infinite thinness.   This is an excellent type of photometer, providing the inner screen is of the right translucency.

Probably the most commonly used type of screen is the Bunsen.   Here the outer portions of the screen are opaque and white, while the central portion is translucent.   Hence, if illuminated from one side and viewed from the other, we should see a bright central spot on a dark ground, while if viewed from one side and illuminated equally on both sides, the surface would appear homogeneous.

There are several methods of producing such screens.   Originally they were simply grease spots on white paper, but more modern discs are constructed by piercing a hole (preferably with serrated edge) in an opaque card, and enclosing this between two translucent sheets of white material, such as parchmentised paper.   The disc is usually mounted in a sighting box with two mirrors, so placed that its sides may be viewed simultaneously, or, better, with Krüss prisms.

The sensitiveness of the Bunsen disc depends to a large extent upon the selection of proper degree of translucency and upon the whiteness of the surfaces of the screen itself.

The beautiful optical modification of the Bunsen screen introduced by Lummer and Brodhun in 1889 has considerably advanced photometric work.   The arrangements of the head are shown in Fig. 9.   The light from the two sources under comparison falls upon the two sides of a pure white diffusing screen; by means of two right-angled prisms the light from each of these surfaces is directed to the compound comparison prism.   This consists of two right-angled prisms, with their longest faces in contact.   One of these faces, however, is ground back in such a way that the contact with the other prism is only made at a central spot.   It will, therefore, at once be obvious that light incident upon the surface A (Fig. 9) will be directly transmitted through the compound prism, while light falling upon B will be totally reflected.

If now these two beams be received in a telescope, we shall see the central portion of the field illuminated by directly transmitted light surrounded by an annulus of light totally reflected.   The division between the two fields is practically an optical one, and hence the photometer ranks as one of the most sensitive.   Indeed, I am strongly of opinion that there is no device that can compare with the photometer for sensitiveness even at very feeble illuminations.   There is, however, one curious defect which seems to be present in all the examples of this instrument I have had an opportunity of examining.

If balance is obtained between two sources of light with this photometer and then the lights are reversed, it will be found that they no longer bear the same ratio to one another.   M. Violle mentions this defect, and puts its value at 3 per cent., but in some examples I have examined it is considerably greater than this.   M. Violle has attributed it to some hitherto unknown property of total reflection.   But my experiments with a screen of this kind show that it is partly due to unequal scattering of light from the surfaces of the prisms C and D, whilst Dr Drysdale has further suggested that the light coming from the white surface does not all reach the totally reflecting surface at an angle greater than the critical angle, and therefore some is transmitted.   It is significant that the light is apparently weakened which is on the side which illuminates the annulus.

Mr A. P. Trotter[1] has devised a simple form of relief photometer (Fig. 10), which consists of two screens of Bristol board which have had their glazed surfaces removed; these are set into a carrier in such a way that each receives illumination from one or other of the two sources, and reflects it through the sighting aperture.   The foremost of these screens entirely fills the field of view, except that in its centre is punched a star-shaped hole through which the back screen may be seen.   When these two screens are equally illuminated, the star-shaped aperture should apparently vanish.   When using such a screen, however, one has considerable difficulty in getting the star to vanish, for it is practically impossible to reduce the edges of the star-shaped aperture to a mere line, and their surface is of a different character to the rest of the screen.   The difficulty, however, is somewhat overcome, I find, by relaxing the accommodation of the eye and thus throwing it out of focus.   This, in fact, forms a very useful method of obtaining balance with any photometer, particularly when the two sources are of slightly

[1] *Proc. Phy. Soc.*, 1893.

different tints. Another difficulty with the Trotter screen is the fact that two roughened white surfaces are continually exposed, and soon become soiled and discoloured; even with the greatest care frequent renewal of the screens is an absolute necessity.

The simple little screen of Dr Joly of Dublin forms an excellent contrast photometer. Here two blocks of translucent material, such as paraffin wax, are mounted together, with an opaque film between them, and light falling on their sides is diffused through them. There is, however, some difficulty in obtaining homogeneous and equally diffusing blocks.

Hitherto the only method used for obtaining balance has been by altering the distance of one or the other lights from the screen, and employing the law of inverse squares in the final calculation of the light.
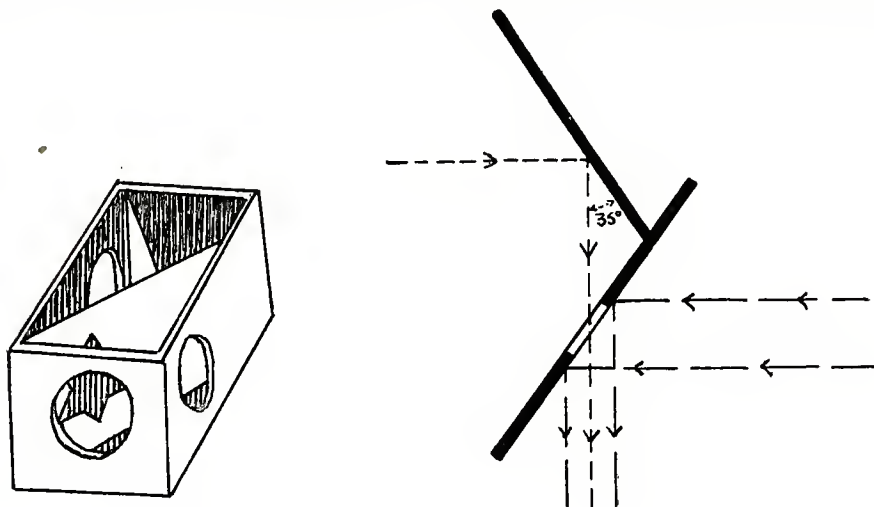
TROTTER PHOTOMETER

Fig. 10.

It, however, frequently happens that it is not possible to do this, or that the light to be measured is so intense as to render the length of bench necessary far too great, and therefore other methods have to be resorted to.

One of the simplest is that originally employed by Fox Talbot in 1834, and which has been extensively employed by Sir W. Abney throughout his work on this subject. It consists of interposing in the path of the intense beam a rotating sector of variable aperture.

If then ϕ is the angular opening of the sector, and I the intensity falling on it, and I_x the intensity it transmits, it is stated that $I_x = I\dfrac{\phi}{360}$.

Now the accuracy of this method has been repeatedly called into question, and many conflicting opinions and experiments have been advanced.

Both Sir W. Abney and General Festing have stated the sectors are accurate; Mr Ferry has stated they are accurate only for lights of the same colour;[1] Prof. Fleming[2] has made experiments confirming their accuracy; while Hurter and Driffield[3] have stated that on account of the semi-shadow at the edges of the aperture, the above formula should be modified as follows:—

$$I_x = I\frac{\phi}{360} + c$$

[1] Cantor Lectures Photometry, 1894.   [2] *Jour. I.E.E.*, vol. xxxii. 1903.
[3] *Jour. Phot. Soc.*, June 20th, 1890, p. 223.

where c is a constant depending upon the position of lamp, sector, and screen, and has but little effect at high intensities, but may rise to 100 per cent. for low ones.

It has further been pointed out by M. Violle that unless the flashes produced are less than ·01 of a second, the results will be in error.

From experiments made by Mr Bull and myself we find that both the last-mentioned errors are present, except that Hurter and Driffield's constant should be preceded by a minus sign—that is, the light transmitted by the sectors is less than the theoretical amount by a certain percentage depending upon the position of lamp and sector. From one calibration the constant c was 4 per cent. for 90° opening, whilst for another it amounted to 10 per cent.

We further observed that as the angular aperture increased the error became larger, but could be reduced by increasing the speed. This is a verification of M. Violle's contention.

Our investigations are not yet completed on this subject, but we feel sure that the sign in Hurter and Driffield's formula must be due to a clerical error, but we have no means of checking this point, as they merely give the formula without figures and with but little comment.

It is interesting, however, in this connection to note that Broca and Sulzer have shown that for intermittent illumination there is an inertia effect on the retina which gives the impression of greater brightness, while two observers in America have arrived at a conclusion which confirms our observations.

There would, therefore, seem to be no doubt that the employment of sectors complicates the physiological effects. But it is possible that their employment in intense parallel beams or in beams from a fairly distant source is permissible if fairly large apertures and high speeds are used.

Table I. gives a set of values for our experiments on this subject.

APERTURE IN DEGREES.	OBSERVED INTENSITY.	CALCULATED INTENSITY.	ERROR PER CENT.	
10	0601	0750	19·87	
22	·1345	·165	18·5	
32	·21	·24	12·5	
45	·295	·337	12·48	
60	·3925	·45	12·77	
90	·605	·675	10·38	
120	·819	·90	9·0	
135	·924	1·012	8·68	
180	1·255	1·34	6·35	

The above observations were made with sectors accurately cut in solid discs, as it was found that the sector opening could not be accurately enough obtained from the adjustable form in use in the laboratory, and further, it was not possible to obtain the speed when using them. From the foregoing, we must therefore conclude that the sectors are not direct reading and require calibrating for each setting of the apparatus, and are quite untrustworthy for apertures less than 10° and greater than 90° unless speed regulation is possible.

Ayrton and Perry have made use of a divergent or concave lens to produce the requisite weakening of the more intense beam, but the reduction of light so produced, if calculated from the known focal length and distances, does take into account the light lost at the surface of the lens in back reflection, and the minor effect of absorption ; these may rise to a considerable value (8 per cent.), and Voller has suggested that since the effect is principally on reflection to compensate for it by inserting in the standard beam a plate of glass of equivalent thickness.

Again we may use diaphragms or stops in conjunction with convergent lenses forming real images of the luminous source since the intensity of the image so formed depends upon the aperture of the

lens and is not altered either in size or position if this aperture be varied. This method has been used by Bouguer, Mascart, Crova, Cornu, and others.

The employment of an absorbing wedge is another solution, and offers many advantages on the score of simplicity and accuracy, providing the light which is being modified is passed through a slit; otherwise the whole cross-section will not be equally weakened. This defect may be overcome by employing two wedges of small angle so mounted to slide one over the other, and hence the thickness may be uniformly varied.

Finally, we may employ the property of polarised light. If a ray of light be polarised by passing it through a Nicol prism and the resulting beam be transmitted through a second and similar Nicol, whose principal section is at an angle with that of the first, the intensity of the emergent ray $Ix = I\cos^2\theta$, where I is the intensity of the beam emerging from the first Nicol. Thus, if the principal sections of the two Nicols be set at $90°$ we have a total extinction, or, if parallel, complete transmission.

The simplest form of polarising photometer is based upon the principle that if ordinary light be allowed to fall upon a pile of plates, set at the polarising angle, the quantity of light polarised in the reflected or refracted beam is equal, and that the planes of polarisation are mutually at right angles.

This principle was employed by Duboscq, who arranged his photometer in such a way that the refracted beam from one source and the reflected beam from the other were received together in a Savart polariscope; if the intensities were equal, each beam would contain an equal amount of polarised light, and since their planes were at right angles, they would behave as ordinary unpolarised light—a condition which was obtained by alteration of the position of one of the sources. Wild has elaborated the above principle and devised a commercial form for which he claims a high degree of accuracy,[1] while many other elaborate and ingenious devices for employing this principle have been suggested, the details of which are too lengthy to be given here.

The principle is, however, when applied to photometry, both valuable and accurate, particularly if the apparatus be provided with the Savart apparatus mentioned above, since one has not to judge equality of illumination but to observe the disappearance of the fringes in the field. It is, however, urged that the continued observance of the fringes is fatiguing to the retina, and further, that the point of disappearance cannot be accurately determined owing to retinal lag, but it is possible to obviate this defect in a measure by giving the eye a rest before making each set of final observations. The fact that in all other forms of photometer one has to balance the illumination on two portions of the field constitutes one of the principal difficulties in photometrical work. For it will be found that there is a comparatively large zone over which the eye is not sufficiently sensitive to detect a difference of illumination, and, further, if the observer observe continuously, it will be found that this zone will gradually increase as the eye becomes more and more fatigued.

Much discussion has arisen out of experiments which have been made to determine the sensitiveness of the eye, but from experiments I have made it would seem that this is a very indefinite quantity, since it will vary within enormous limits in a single individual. Masson claims to have distinguished $\frac{1}{180}$ of an illumination of the order of diffused daylight, Helmholtz $\frac{1}{133}$ and at times $\frac{1}{150}$, with a maximum of $\frac{1}{167}$, while Fechner, Arago, and Bouguer give values much below these.

Weber and Fechner have endeavoured to find the relation between the excitation of the retina and the magnitude of the visual sensation, and the latter has given the law :—

That the intensity of the sensation is proportional to the logarithm of the excitation.

Broca, however, insists that no relation can be stated unless the condition of the eye at the moment is carefully specified. But owing to constant changes going on in the organ and its continual reconstitution it tends under a definite excitation to a limit. Further, it will be found that the sensitiveness of any photometric arrangement will depend to a large extent upon the effective illumination at the photometer screen, and therefore such photometers as shadow photometers which cut down this illumination lose

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[1] *Pogg Annal*, cxviii., p. 193.

sensitiveness enormously at low luminosities. Broca has suggested that the best illumination is about 1 carcel metre, while Masson and others have suggested an illumination of the order of diffused daylight. In this connection one should refer to the theory put forward by J. V. Kries that it is the rod elements of the retina which convey to the brain a colourless sensation of brightness while the cones are the colour-perceiving elements.

Upon this theory Dr Lummer has explained the first grey glow of a heated body discovered by Weber by assuming that the rods are more sensitive to feeble illuminations than the cones. This theory has been further supported by experiments of allowing the light from a coloured object to fall on the periphery of the retina where the rods are more plentiful, and it is then found that the colour disappears. But according to Dr Ettles this is not so if the object is a small but intense luminous source, such as a small coloured glow lamp, whose colour can always be defined as long as it can be seen.

The theory finds further support in the fact that the eyes of nocturnal animals are almost devoid of cones, yet their eyes must necessarily be more sensitive to light than ours.

But in vision, as in most other phenomena of similar nature, there is a minimum stimulus which is capable of exciting the retina—this is known as the stimulus threshold—while on the other hand there is a maximum limit beyond which we are incapable of appreciating any difference of illumination. We may therefore state Fechner's law in the following fashion :—The sensation varies as the logarithm of the ratio of stimulus to minimum stimulus. We therefore see that all photometers will be insensitive about their zero positions for low intensities. Sir W. Abney has suggested that this may be overcome by oscillation of one of the lights over continuously decreasing lengths. But this method does not work well with all observers.

There is, however, another method that I have used with considerable success, and which has proved successful with many other observers.

This method consists of moving the photometer or each of the lamps until the first perceptible difference is observed on the photometer field ; this then determines each end of the neutral zone, and a mean of the two readings gives the balance position with considerable accuracy. It will be found that a difference in the uniformity of the field can be detected with considerable exactness.

Broca has also pointed out that the effects of the two eyes are additive, and therefore increased sensitiveness is obtained by making all photometers binocular ; he suggests that monocular photometers should be provided with a Wollaston doubling Camera Lucida.

Further, nearly all photometers employ diffusing surfaces which do not scatter the light equally in all directions, hence, unless the head is fixed in position relatively to the screen, discordant results will be obtained due to unequal scattering. This point is one of the causes of the shadow photometer giving unreliable results, and if the photometer be provided with an attachment which fixes the head for all positions, the results are greatly improved.

Heterochromatic Photometry.—In considering the foregoing processes, we have assumed the two lights to be always of the same spectral composition. This, however, if the lights be produced by different methods, is seldom the case, and we must therefore resort to some process which will eliminate the resulting colour difference.

We may regard a light source as interesting from two points of view, viz.—from its luminous intensity or from its visual intensity. These two values are regarded by MM. Macé de Lépinay and Nicati, Dr Charpentier, MM. Blondel and Langley to be entirely independent (Fig. 11).

According to M. Lépinay, if we balance the objective brightness of a surface, half of which is illuminated by a blue light and half by a red light, then a printed page will be more easily read by the red illumination than by the blue. But it should be remarked that this experiment depends upon obtaining the first balance of the objective brightness of the two lights, a process attended by great difficulty and but little accuracy. Dr Charpentier has stated that vision comprises three distinct processes : 1. The preception of light ; 2. The picking out of detail ; 3. The preception of colouration.

Now, if we agree to define the luminosity of a surface as a measure of the light energy it sends to

the eye and producing a definite sensation on the retina, then as long as the lights have the same spectral composition we are independent of any physiological complications. But as soon as any difference of spectral quality occurs it will be found that the sensation is not the same for different wave-lengths having the same luminosity. This is the phenomena discovered by Purkinje and enunciated by Helmholtz, who has stated it in the following form.

The intensity of sensation is a function of the luminous intensity, and varies according to a different law for each colour, the rate of change of the sensation being greatest for the red ray and least for the blue (Fig. 12). Blondel, in discussing this phenomena in the light of Kries' theory mentioned above, supposes the sensibility of the cones to diminish more rapidly than the rods as the intensity diminishes. He further suggests that if the field of vision be limited to the yellow spot the Purkinje phenomena is
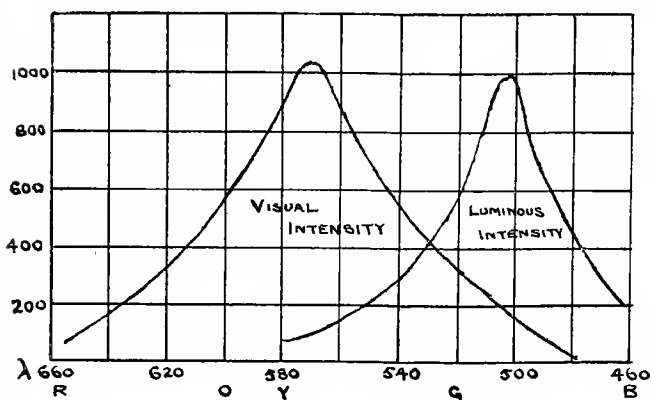
DISTRIBUTION OF LUMINOUS & VISUAL INTENSITY
IN SPECTRUM.   (CHARPENTIER)
Fig. 11.
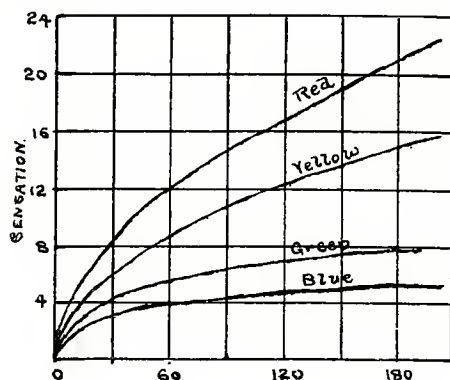
SENSATION CURVES FOR COLOURS
(CHARPENTIER)
Fig. 12.

diminished, since at this part of the retina no rods are present, and hence the comparison of coloured lights becomes simpler.

Dr Lummer has further demonstrated Kries' theory in the following way : He fixes a blue, green, and a red rectangular patch on a blackened screen illuminated by an arc, and arranges that the two appear initially equally bright. Into the path of the light he interposes two Nicol prisms. If one of the prisms now be turned, the red patch is found to grow darker as the illumination diminishes, and the green one will stand out in bolder relief. Finally, the red will disappear and the green will have lost its colour, appearing whitish ; if now the experiment be repeated but with a diaphragm interposed so that only a small red and green spot is visible, the two colours may be differentiated as long as anything can be seen at all, but on removal of the diaphragm the red patch disappears and the green looses its distinctive colour.

It would therefore appear that a surface illuminated by a red and a blue light, each of the same physical intensity, produce different physiological sensations in the eye, and therefore the balancing of the objective brightness of a surface, a process always attended by considerable difficulty, is of little value.

The relative sensitiveness of the eye to different colours still remains to be determined with accuracy. Ebert has given some values for various wave-lengths, which show a maximum excitation in the green and a minimum in the red, and this result has been confirmed by Langley.

For these reasons, Von Helmholtz has stated that eye comparisons of intensities of different kinds of composite lights cannot give objective values since none of them are independent of the nature of the eye itself.

Now from a utilitarian point of view the primary object of vision is the discrimination of detail. If we illuminate a printed page or ruled pattern of black lines on a white ground, then the ease with which we can pick out the details of the pattern or read the print will depend upon the intensity of the illumination falling on the object. Hence we may make this phenomena the basis of our photometric comparisons, and say that our two sources of light are equal when their light, falling on a black and white diagram whose elements do not subtend an angle less than 1' at the eye of the observer, allow him to distinguish the details of it with equal amount of ease. This method was originally used by Celsius and Herschell, and has been carefully investigated by Macé de Lépinay and Nicati. In principle it may be applied to any photometer where illuminations on two adjacent surfaces are to be compared.

If we provide the surfaces with a ruled diagram such as Fig. 13, and view this diagram at the distance of distinct vision, the lines being $\frac{1}{15}$ mm. wide, we at once convert an intensity photometer into a distinctness photometer.

Carter has applied the principle to a Joly paraffin block photometer, Prof. Fleming to a simple wedge photometer. The author has found a photometer constructed on the plan of the photoped, and provided with a discrimination diagram, work with great exactness.

There is, however, one very important consideration in connection with the subject, which was first pointed out by Prof. Fleming, and is really the whole crux of the method.

If the illumination on the discrimination diagram diminishes, the pupil of the eye expands in the effort to discern detail, and hence the point of disappearance is not sharp.

Prof. Fleming has overcome this trouble by viewing the diagram through an artificial pupil consisting of a hole pierced in a metal diaphragm.

If such a diaphragm be used, it will be found that the point of distinction is quite sharp, and, further, it has the advantage that it fixes the position of head, and thus overcomes the difficulty of uneven scattering at the diffusing surface previously referred to.

DISCRIMINATION   DIAGRAM
Fig. 13.

The question as to which is the right method to employ in hetrochromatric photometry has yet to be decided. But one thing is certain, the detail revealing power for any given source of light does follow the law of inverse squares for lights whose component wave-lengths constitute comparatively broad bands in the spectrum, and the discernment of detail is the primary function of vision.

Macé de Lépinay and Weber have employed methods which consist of determining the intensity of the red and green components by the use of liquid filters. It is interesting to note that Weber, in his re-determination of Lépinay's function, upon which the whole method depends, used a distinctness method. Crova's method depended upon the fact enunciated by him that the brightness of two lights which are nearly white are in the ratio of the brightness of the rays in them of wave-length $\cdot582\mu$. He therefore used a special filter, consisting of a solution of the following composition :

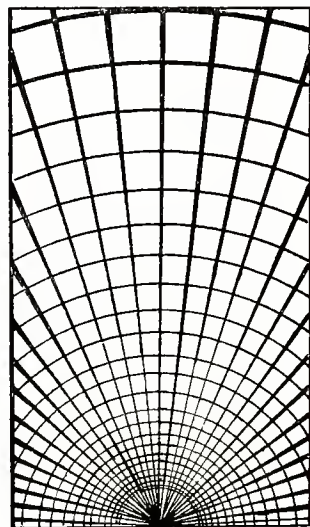Anhydrous sublimed perchloride of iron   .   .   .   22·321 grms
Crystallized chloride of nickel   .   .   .   .   .   27·191   ,,

dissolved in distilled water, and the volume made up to 100 cc. at 15° C.

A thickness of 7 mm. passes wave-lengths ·630 to 534μ, and these limits approach one another as the thickness increases. The filter should be kept at 13° C. The photometer screen was adjusted to equality when viewed through this filter. The method is restricted to lights not of very widely differing spectral quality.

In 1893 Prof. O. W. Rood suggested another method of eliminating colour difficulties.[1]

[1] *Am. Jour. Science*, vol. lxvi. 1893. *Science*, vol. vii. 1898.

This method consists of alternately stimulating the retina with light reflected from each of the two sources under comparison and modifying one or both until flicker disappears from the field. His photometer consisted of a wedge whose surfaces could be alternately viewed by an oscillating lens or prism.

Several modifications of this type of photometer have been suggested. Thus Bechstein has alternately viewed the sides of a right angled prism illuminated by the two sources of light by rotating a tilted and decentred lens before it, while in a further modification 30° prism is rotated and alternately reflects the light from each source into the eye-piece.

Prof. Whitman has proposed a simple form of flicker photometer in which a fixed white surface is illuminated by one of the sources of light. This screen is set up at an angle of 60° to the axis of an observing telescope. In front of this fixed screen is placed a motor driven sector also of white card, this sector being illuminated from the second source of light. With this arrangement one obtains glimpses of the back card, and then the sector alternately, and the lights are adjusted until flicker vanishes. Prof. Fleming has modified the above arrangement by splitting the sector up into a set of four sectors, like a Maltese cross.

Krüss has evolved a form of flicker photometer which consists of a rotating disc. On the periphery are a set of mirrors mounted at 45° to the axis, each mirror reflecting light from the right and left alternately, whilst this principle has been employed in a much simpler fashion by Messrs Simmance and Abady in this country.[1] Their photometer consists of a disc of white material whose periphery is turned to form two equal conical surfaces. The axes of these cones are parallel to the axis of rotation, but displaced on each side of it by equal amounts. The periphery is thus formed by two surfaces sloping at equal but opposite angles, and the separating ridge formed by their intersection, crosses and re-crosses the field of view in each revolution.

It will be seen that in all the above flicker devices the object aimed at is to balance equal luminous sensations. And it would seem theoretically that the method cannot fail to be successful. Indeed, it will be found that for lights of the same tint, or of nearly the same tint, the methods are capable of yielding excellent results.

Lauriol, investigating the matter, has given a speed of 6 to 8 revolutions per second for the Abady photometer. But he asserts that no certain results can be obtained with this instrument if the lights are of dissimilar colour, owing to complications due to physiological and speed phenomena. The author has also made a number of comparisons with this photometer and obtained the following results. With lights which approximate to white and which are, therefore, not of very dissimilar colour, the photometer run at a slow speed gives results which compare very favourably with those taken with an ordinary Bunsen screen. But the sensitiveness is less than that of a good Lummer Brodhun head. With lights of dissimilar colours but of the same visual intensity, the photometer is much less sensitive and flicker reaches a minimum but never disappears, while for comparisons of a narrow band in the spectrum with white light the results are quite untrustworthy. Messrs Simmance and Abady have claimed that their photometer is independent of the Purkinje phenomena, but it is difficult to follow their arguments, since the photometer essentially is based upon the balance of two sensations, and there is but little doubt that for equal luminosities the red and blue sensations have different values. In this connection, however, Broca and Sulzer have shown that if a coloured light be allowed to act on the eye for successive brief periods the brightness of the intermittent light is apparently increased, the blue being most affected, the red intermediate, and the green least. From this they conclude that blue causes greatest retinal fatigue and green least.

Of photometers which aspire to be independent of the eye altogether very little need be said, for, as has been mentioned before, light is for purposes of vision, and these photometers must find their original calibration by some visual means, and, like all calibrated instruments, they would require frequently checking if their indications are to be relied upon.

[1] *Proc Phys. Soc.*, vol. xix., May 1904.

The employment of selenium, which alters in resistance when exposed to light, has not up to the present been entirely successful, whilst chemical photometers, which depend upon the combination or reduction of chemical substances under the action of light, are very uncertain in their indications, and are very largely affected by ultra violet light.

It would, therefore, seem that a method is still wanting by which the luminous intensity of a coloured light may be determined if this quantity is differentiated from the visual intensity; and, further, that the visual intensity can be more exactly measured by a discrimination device for lights of widely different spectral quality than can their luminous intensity by any of the methods mentioned above.

It is interesting to note, in connection with the above, that Sir W. Abney has stated repeatedly that, using the sectors and the shadow photometer, he is able to measure the luminous intensity of two or three coloured patches from the spectrum in terms of white light from the same source and to combine them into a white patch, and finds that the sum of the separate luminosities are equal to the luminosity of the resultant white patch to a surprising degree of accuracy.

Mr Bull and the author are endeavouring to repeat these results, but up to the present have not been very successful, although the apparatus we are employing is practically identical with Sir W. Abney's.

The above methods are all based upon the comparison of the general impression of brightness of the surface illuminated by the coloured beam.

For the exact comparison of two luminous sources of different quality we must resort to spectro-photometry, and compare the ratio of the intensity of the individual component rays, one with the other. The process is, in consequence, a difficult and tedious one.

In all cases the two sources of light are decomposed into their spectrum by means of some optical dispersive device, such as a prism train or a diffraction grating, and the two spectra are then compared in detail, the luminosity of the comparison spectrum being usually adjusted by crossed Nicols or by altering the apertures of the slit through which the light enters, the slit aperture being, of course, accurately measurable. This latter device, however, is not of great accuracy, especially where wide apertures have to be reached. Trannin[1] polarised both beams of light before entering the slit at right angles to one another, and balanced them by noting the disappearance of the complementary fringes produced by suitable optical devices. The instruments of Glans and of Crova are of a similar nature.

Most of the modern instruments employ one or the other of the above principles. But the subject is too intricate and highly specialised to allow of detailed treatment here.

In conclusion, although I cannot claim to have done more than bring before you the leading features of this subject, I trust that, in presenting them in a collected form, they may prove of service to those specially interested in this subject, and indicate directions in which special optical investigation would prove valuable.

Finally, the author wishes to tender his thanks to Dr Drysdale, at whose suggestion the paper was written, and who has given the author much valuable advice during its compilation.

Dr C. V. DRYSDALE would have preferred that the opening remarks of the discussion should have come from some one better qualified to take part in the proceedings, but he supposed he had been asked to speak first because Mr Jolley has referred to him as having had some small share in the preparation of the paper. In the first place, he should like to disclaim that altogether. He was very much in agreement with Mr Jolley, and no doubt they had talked a good many things over together, but he wished it to be understood that Mr Jolley's paper was his own work, and considered that it was very valuable work indeed. The subject of photometry was probably the most important one that could possibly be brought before this Convention. The position was that an enormous amount of work had been done on the subject, but the work had been done in

[1] *Jour. de Phys.*, vol. v., p. 297.

a large number of different places by different observers working quite independently, the result of their work had been scattered in various scientific periodicals, and it was very essential that this congress should have a paper brought before it in which the work was summarised. This had, he thought, been admirably done by Mr Jolley, and he felt sure that the discussion upon the paper would settle some points of extreme practical importance. Dr Drysdale confined himself to what he called the practical engineer's standpoint, rather than the scientific standpoint. The points that he desired to deal with more particularly were, in the first place, the extreme importance of heterochromatic photometry, or the photometry of lights of different colours. A few years ago we were not so very much concerned with that question. When illumination was produced by means of candles or gas flames or electric glow lamps which had approximately the same colour there was not much trouble, but as we were now getting such widely diverse means of illumination, such as the mercury vapour lamp and the flame arc lamp, the one with a bluish-green, and another with an orange colour, it became of great importance to compare such sources of light with the view of ascertaining their relative efficiency. Under these circumstances heterochromatic photometry and the question of what our standard procedure should be, had become of the first importance. The difficulty was a question of definition, and Dr Drysdale thought that in photometry as a rule sufficient attention had not been given to that point. In the comparison of different coloured lights, what did we wish to compare? Mr Jolley had put the issue clearly. We were told there are two ways in which lights may be compared, either with a view to considering their intensity of illumination, or in order to consider the power of discrimination. He (Dr Drysdale) wished to impress most particularly on the Convention that in his opinion the discrimination test was *the* test. The test of light, until we had something better, was its utility to us, and lights had equal utility to us when they gave us equal power of discrimination. No doubt there was some other physiological effect of illumination besides, but he did not think it capable of being put into any quantitative form at the present time, and until that was the case discrimination ought to be our guide. Therefore, the discrimination method should be adopted first of all; and other methods which might possibly be more convenient must only be used in so far as they gave results in accord with that method.

The difficulty in connection with discrimination tests which had been referred to in the paper, was that of the pupillary variation. The idea of the artificial pupil suggested by Dr Fleming was excellent, but there was a difficulty connected with it, namely, the question as to whether the artificial limitation of the pupil in that case was fair, and whether the pupil might not take two different diameters for the two different coloured lights. If this was not the case then the artificial pupil as applied to the discrimination test seemed to remove the only real difficulty in connection with it, and Dr Drysdale hoped to see this adopted as the standard method.

One other difficulty in heterochromatic work presented itself, but it could easily be disposed of. It had been repeatedly pointed out that illuminations of very different colour which appeared of equal intensity, no longer appeared equal if these illuminations were increased in equal ratio. This effect, known as the Purkinje phenomenon, could, however, be eliminated by specifying the illumination at which the comparison was to be effected, and this illumination should be, of course, one which was a useful all round amount. Now the illumination given by a 10-candle pentane standard at a metre, which was approximately the same as a candle foot, was a very satisfactory amount for artificial illumination, and the lamp metre had been proposed by Dr Fleming as the unit of illumination.

If, then, the discrimination test is adopted with an illumination of one lamp metre (easily effected by keeping the standard at a fixed distance from the photometer screen) it appeared that the conditions would be satisfactory for all classes of lamps.

With reference to the flicker photometer many observers appeared to have found that great definiteness could be obtained in balancing with different coloured lights. As this was the case, and Messrs Simmance and Abady had stated that it gives the same results as the discrimination test, it

might be very valuable for general use, but it would be advisable to have a large body of evidence as to whether this agreement was quite satisfactory, as the personal element was so important.

Another point raised by Mr Jolley in his paper concerned the validity of the use of rotating sectors as a means of quantitatively varying the intensity of a beam of light. This method was so convenient in some cases that precise information as to the justification for its use would be welcome.

Notwithstanding the powerful advocacy of Sir W. Abney, and the confirmation of Professor Fleming, there yet was reason to doubt whether these sectors were accurate under all conditions. Dr Drysdale had made no experimental tests on the matter but wished to adduce a theoretical consideration tending to show that the simple law of the sectors could not be expected to hold unless under exceptional conditions.

If I is the intensity of brightness of any surface at any instant and S the corresponding visual intensity, S is some function of I, say $S = f(I)$ or $I = f^{-1}(S)$, where f^{-1} is the function inverse to f. Hence, if I varies according to a periodic law of such frequency that the variations are imperceptible we have :

$$\text{mean } S = \text{mean } f(I)$$

But if a second surface has an actual intensity of I^1 the corresponding visual intensity $S^1 = f(I^1)$. Hence if the surfaces appear equally bright to the eye $S^1 = \text{mean } S$

$$\text{or } f(I^1) = \text{mean } f(I)$$

and $I^1 = f^{-1}(\text{mean } f(I)) = f^{-1}\left(\dfrac{1}{T}\int_0^T f(I)\, dt\right)$ since $\text{mean } f(I) = \dfrac{1}{T}\int_0^T f(I)\, dt$, where T is the periodic time of variation.

Now the ordinary formula for the rotating sectors assumes that the resultant $I'' = \text{mean } I = \dfrac{1}{T}\int_0^T I\, dt$.

Consequently we have $\dfrac{I'}{I''} = \dfrac{f^{-1}\left(\text{mean } f(I)\right)}{\text{mean } I} = \dfrac{f^{-1}\left(\dfrac{1}{T}\int_0^T f(I)\, dt\right)}{\dfrac{1}{T}\int_0^T I\, dt}$

the value of which will depend upon the form of the function f and *upon the mode of variation of I*[1]. This ratio may be called the visual form factor, and is analogous to the form factor used in alternate current work, which is represented by $\dfrac{\sqrt{\text{mean square}}}{\text{mean}}$. Now we do not know the form of the function f for rapid variations in the eye, but the importance of this investigation is that it shows that the form factor can only be unity if, *over the range of variation, the sensation is proportional to the stimulus*. If the sensation increases at a greater rate than the stimulus, the visual form factor will be *greater than unity*, and the illumination will appear greater than that given by the ordinary formula for the sectors, despite the apparent paradox; while if the sensation increases more slowly than the stimulus, as is certainly the case for slow variations, the form factor will be less than unity, and the apparent illumination will be lower than that assumed, as Mr Jolley has found. The assumption of the correctness of the sectors therefore implies the proportionality of sensation to stimulus in the eye for rapid variations, and since this is contrary to what we have reason to expect, it is incumbent on those who support the assumption to prove their contention by exhaustive experiments over a wide range. The range is of great importance, as there is some reason to believe that the curve of sensation to stimulus is inflected, and the form factor may be less, equal, or greater than unity, depending upon the part of the curve over which the measurements are made. By such experiments with a known law of variation it should be possible to obtain the law of stimulus to sensation by differentiation.

Dr Drysdale concluded by expressing the hope that a committee of the Convention might report on this subject of photometric measurements with a view to securing a concensus of opinion on these debateable points.

[1] This was experimentally found by Messrs Hurter and Driffield.

Mr BULL had noticed that in matching dissimilar colours by the Lummer-Brodhun photometer—perhaps one of the best forms of the luminous intensity photometer—the eye tended to assume an arbitrary, but for the time repeatable, relation between them. This might be a reason, in addition to individual differences of colour sensibility, why two observers would obtain consistently differing readings.

It was quite possible, as Mr Jolley would seem to think, that more consistent readings might be obtained by the use of discrimination photometers, which, although they might not measure luminous intensity, might very possibly yield results more in accord with the usual uses of vision.

With respect to flicker photometers, Mr Bull had noted the large neutral zone mentioned by Mr Jolley as occurring with the Simmance-Abady form of the instrument, and would like to ask his opinion as to whether he did not think that a form of flicker wheel which has been in this Institute for the last six or seven years would not be a better form. It has one semi-circumference bevelled at 45° one way, and the other bevelled at 45° in the reverse direction, with an abrupt transition between them.

A point of which account must be taken in the comparison of dissimilarly coloured lights was that not only was the luminous effect not proportional to the amount of light, but that the relation varied considerably with the wave-length. The result of this was that no simple arithmetical relations existed except perhaps at certain somewhat low luminosities, and then only over a very limited range. This variation, with wave-length, of the shape of the curve, giving the relation between light stimulus and visual effect, was shown to exist by the Purkinje effect, and by the variation of the hue of both compound and mono-chromatic colours with brightness. It was, therefore, quite possible that in order to obtain comparative results, account might have to be taken of the brightness of the illumination at the photo-meter and the colour sensitiveness of the eye of the operator.

A practical difficulty in some colour work was that the inverse square law could not be employed to vary the illumination at the photometer, and among the various other methods that could be used, the rotating sectors appeared to Mr Bull to be undesirable. From experiments which Mr Jolley and he had made, it would appear that the apparent luminosity is a function of the speed, but that at a given speed the readings were proportional among themselves, although diminished by a constant coefficient. Other observers had arrived at different results, some giving positive coefficients, others negative ones, and others again finding that the sectors read correctly. The chief reason which led Mr Jolley and Mr Bull to examine the rotating sectors carefully was that from a consideration of molecular phenomena in relation to vision there was no reason to expect the eye to correctly integrate intermittent illumination. To do this the lag in taking up the sensation would have to equal the recovery period, a thing which practically never happened with any molecular phenomenon.

Mr W. ROSENHAIN : A great many points arise in this connection which are of general interest. One suggestion may be worth thinking over, and that is whether it is not possible to get an absolute unit of light. Something in this direction has been done in Germany already. The idea, of course, is to find the heat equivalent of the light, and there are many ways of doing that which suggest themselves. For instance, it would be possible to enclose an electric glow-lamp in a calorimeter—first of all, a transparent calorimeter, and then an opaque one—and see whether you can get any difference in the amount of heat absorbed in a given time. Another plan would be to send a beam of light round and round the calori-meter by a reflection method. Of course, it is a very delicate matter, and one which experimenters would be rather slow to take up. At any rate, theoretically, you have a possibility of finding the heat value of light with a given photometric intensity, and this would be the absolute value in terms of the actual energy of the beam of light in question. It would, of course, be difficult to say exactly what radiations were transmitted or absorbed by the calorimeter, but it might, I think, be possible to get a value for the fundamental unit in some such way. The whole subject of photometry is so complicated and beset with such various classes of difficulty—optical, geometrical, physical, and physiological—that really when you try to define such a thing as the candle-power of a lighthouse beam, it is misleading to

give any figures on the subject.   Figures are used by lighthouse engineers, but they have to be taken very frequently as efforts of the imagination ; because, when you come to apply the inverse square law to an area such as that of the lenses in the case of a 10-ft. beam, and have to make your measurements either at great distances, with an unknown amount of absorption, or at small distances, with considerable departures from the inverse square law, on the whole the less said about the candle-power the better. The final test is how far you can see the light on a foggy night.

Mr J. R. MILNE commented on Mr Jolley's classification of objects of photometric measurements. Mr Jolly had said : " We may wish to determine the amount of light reflected, transmitted, or absorbed, by a given substance or optical system."   And at the foot of the clause, " It will be obvious that before any of the above objects can be achieved we must institute some unit, and create a standard representing that unit."   He thought this was likely to be misleading, because as regards absorptive photometry that is exactly what was not required.   That was one of the great advantages, that no unit was required ; the original light, whatever be its power, was simply compared with the light which had been absorbed by the given substance.   The fourth class, the distribution of light, likewise did not require the institution of a unit, because the light in a particular direction being taken as unity, the intensity in all the other directions could be given in terms of the first as a standard.   He drew attention to the work of Prof. Swan, which seems to have been very largely forgotten.   For example, what was popularly called the Lummer-Brodhun prism was originally invented by Prof. Swan in the year 1859, who described it in the " Proceedings of the Royal Society of Edinburgh " (4th April).   For some unexplained reason his work had been largely lost sight of, and the credit for that very brilliant and useful piece of apparatus had gone to Germany.   That same paper contained a record of some of the earliest experiments which were made on the effect of intermittent illumination on the retina, and results arising therefrom, which are very pertinent to the question of the rotating sector.

Mr ANDREWS : As one engaged in the daily examination of other people's sight, I should like to say that any test of photometric intensity that depends upon individual eyesight is, to my mind, bound to fail, especially with regard to coloured light.   Many people are more or less colour-blind, and I am afraid a test depending on the human eye will fail, because you must know whose eye is making the test before you can compare the result obtained by one man with the result obtained by another.

Mr C. M. DOWSE : As secondary standards, to be checked against a melted platinum standard, the incandescent lamp of Prof. Fleming seems to be most satisfactory.   It is portable and is ready for use in a few moments, whereas flame standards need a considerable time to settle down to a constant value; moreover, the errors due to flame height, atmospheric impurities (especially CO_2 the amount of which is difficult to determine) and quality of fuel all tend to place the incandescent lamp standard easily first for convenience.   The arrangements at the National Physical Laboratory have proved that the necessary electrical adjustments can be made with ease and accuracy, and therefore for all ordinary purposes electric lamps as standards should prove most accurate and useful.

One disadvantage of the distinctness photometer is the greater retinal and general physiological fatigue which must necessarily result from the continued effort to discern minute details and separate lines upon the diagram.   This will cause a period of insensitiveness to set in and limit the period of accurate observations, whereas the effort necessary to distinguish a difference of illumination over a fairly large area, as in the Lummer-Brodhun photometer, is not so fatiguing.

Some few years ago he had considerable experience with the Lummer-Brodhun photometer, in testing acetylene flames and arc lamps.   The head was of the " contrast " type, and was so sensitive that while a standard Methven screen was being used, very small variations in gas pressure or almost invisible movements of the flame due to draughts, could both be easily seen while watching the illuminated pattern.   The greatest error which resulted from reversing the positions of standard and test lights during fifteen observations, was found to be 3·5 per cent. and the mean error 2 per cent.

The CHAIRMAN, having pointed out that time for closing had passed, said there was no doubt, he thought, that Hurter and Driffield were wrong in what they said with regard to sectors, and in the particular form in which they put their equation. It is perfectly certain that you cannot get more light through a sector than goes into it, and that is really what Hurter and Driffield apparently did, as he had pointed out to them. After that paper had been read, Dr Hurter visited him at his laboratory and expressed himself as satisfied with the correctness of the results given by the sectors used. In America, also, some years ago, doubt was thrown upon the accuracy of the sector. Having used sectors a good deal, he had investigated the question, and the conclusion was that sectors, so long as you have sufficient rotation, are perfectly accurate. He had tested sectors from time to time by other methods, such as wedges, moving lights, and so on; and could not understand how an error of 10 per cent. was possible, unless there were something very wrong with the sectors. It is possible that a very small percentage of such effect might be present due to thickness, but for practical purposes he had failed to find that amount of error in the way in which he used the sectors. Those who use sectors need not lose faith in them if they use them in the proper way.

SIR W. ABNEY (*communicated*). When the above paper was read I occupied the position of chairman of the meeting, and as such, I had to reserve any remarks I had to make till the last speaker had finished. When I rose, the time for closing the meeting had arrived, and though I had prepared some criticisms on parts of the paper, I had to omit many points which would have required some time to bring out. Having been asked if I wished to add in writing any criticisms, I have considered it better to defer them, in detail, for an opportunity which will shortly arise. I may say, indeed I have said, that as long as the author confined himself to the historical side of the question, the information, if slightly incomplete, is very valuable, but to his personal views on other matters, I think I am justified in demurring, more particularly as he introduces myself as holding contrary views to his own. It is stated, for instance, that I have used exclusively the Romford photometer. This is not the case; but I have been driven to the conclusion that it is, at least, as sensitive, when properly used, as the Lummer and Brodhun photometer. I am not alone in this opinion. Again, the accuracy of sectors is disputed, and the author suggests by his remarks, but I am sure not intentionally, that Mr Festing and myself took no trouble to ascertain the truth of the readings given by them. The surprising cause of sector errors in readings, which the author showed, wanted ample discussion. Without entering into details as to the many methods taken to check the sector results, I may say that such an error, when the sectors are properly used, is impossible. I doubt if Mr Jolley would give the recommendation to the polarising method of comparing intensities, if he were acquainted with certain criticisms which appeared a few years ago at a learned society. In regard to hetrochromatic photometry, I only wish to refer him to Colour Photometry, Part III., in the Phil. Trans. of the Royal Society, where he will find measures of what may be called the Purkinje phenomena. In regard to what has to be measured in hetrochromatic photometry, I see that the author regards what is called acuteness of vision as the quality to be measured. This is so dependent on the optical as well as physiological part of the eye that it becomes misleading, and additive measures of different colours do not bear out the measures made by the same colours mixed, whilst those made by measuring luminosities do, though I regret to read that Mr Bull and the author have not yet found it so. If the sectors used were those which gave the large source of error, I should be surprised if they had not failed. I should like to suggest that Professor Draper's name ought to be mentioned in connection with flicker photometry.

I should like to say here that Dr Drysdale's theoretical consideration of the light through rotating sectors requires further discussion, but perhaps the accuracy of the readings of the sector may enable the function f to be determined.

I have added these scant remarks in order that it may not be considered I accepted the author's dictum as to the main debatable points, though there are others to which I demur.

Mr JOLLEY (in reply) said : I am glad that Dr Drysdale so strongly advocates discrimination methods of photometry.   And it will be found that even with methods which profess to measure luminous intensity the final adjudgment of balance is often influenced by the amount of detail one unconsciously searches for even on a plain photometer screen.   Dr Drysdale has raised the question of the validity of the artificial pupil.   The point raised is an important one, and requires further experimental work ; but with a standardised pupil and discrimination pattern results are certainly very concordant.   The final mathematical investigation of the rotating sectors is a very valuable contribution to the subject, and is fully confirmed by the experimental results which I had obtained at the time of writing the paper, and by those at present in hand.

Mr Bull refers to the form of flicker-wheel which was tried some years ago at the Northampton Institute.   This instrument was constructed at the suggestion of Dr Drysdale in 1897, but the sharp dividing line used did not allow of flicker entirely disappearing, and the method was therefore abandoned.

Mr Rosenhain suggests the establishment of a light standard based upon the determination of the heat equivalent of light ; but, as I have previously pointed out, all measurements to be of any value must be visual ones, and hence such a standard, which is independent of the eye, would not be of great practical value.   Nevertheless, many valuable measurements of luminous efficiency of various light sources have been made from time to time on the plan suggested by Mr Rosenhain.

Mr Milne has pointed out that it is unnecessary to have a standard in absorptive photometry or in the measurement of distribution of light.   In the first case I agree that a standard is not necessary, although one must employ in many instances some constant source of light, which is usually a secondary standard.   This argument applies also to the second case, viz., distribution of light ; but in this case, if the distribution curves are to be of their greatest value, it is certainly necessary to standardise the comparison beam.   I must thank Mr Milne for drawing my attention to Prof. Swan's paper.   I much regret not having mentioned him in connection with the prism photometer, although I was aware of his invention through the correspondence on this subject in the columns of *Nature* some years ago.

Mr Andrews has questioned the use of discrimination methods from the sight-testing optician's standpoint.   But there are two points to be considered in this connection.   First, since the eye is to be the standard instrument, we can only tolerate a healthy and perfect organ.   Secondly, the diagram has only half its elements illuminated from each source of light, and balance is obtained when corresponding elements of each half are equally distinct.   Hence, errors of discernment will affect each equally.

Mr Dowse claims that retinal fatigue is greater with discrimination photometers.   But my own experience is contrary to this, and I assume that this is partly because the amount of light falling on the retina is less.   Further, physiological fatigue can be entirely eliminated if one does not observe or try to observe, detail requiring a great effort.   The diagram is a graduated one, and it is not necessary to observe the smallest elements of it, if to do so costs an effort.   Its object is achieved if similar elements are equally clear.

In reply to Sir W. Abney, I should like to say that I did not intend to convey the impression that he had not used any form of photometer other than the shadow, but that in practically all his communications on this subject he had strongly advocated its use.   It would therefore be interesting in this connection if at some future date further details could be given of the methods of using the apparatus which render it as sensitive as the Lummer-Brodhun screen.   Sir W. Abney has questioned my figures for the rotating sectors, and I should therefore like to amplify what I have already said on this matter.

The error seems to arise from two causes : one due to the nature of the flash itself, the other due to the speed of rotation of the sectors.   The first is a constant error, and affects the photometric values *only when the intensity of the beam before passing through the sectors is considered.*   This is important, as many tests have been conducted where the *ratio of the sector aperture only* is considered, and under

these conditions the sectors will appear to give the correct reduction of the beam, since the error is practically constant over the small range of apertures usually used.

The error due to this cause may be modified in many ways, as, for instance, by splitting the aperture into a number of smaller components, or by altering the form and distance of the source of light, and in this way altering what Dr Drysdale has called the form factor of the flash.

The speed error is also important, and has not hitherto been sufficiently emphasised. It is interesting to note that if we are to reach the limit of ·01 second as the time of one flash with sectors of the double aperture type usually employed for this purpose, they must be driven at a speed not less than 1500 revolutions per minute for a 90° aperture. The sectors used to obtain the results published were specially cut, and mounted direct on the spindle of an electro-motor after their angular apertures had been carefully ascertained, a separate disc being used for each aperture.

I do not think that acuteness of vision is measured by a discrimination photometer, for this quantity (acuteness) will determine how small an element the eye can discern, while in the discrimination photometer, as I have already pointed out, the balance is decided by the equal distinctness of equal elements illuminated by the two sources, the size of the elements being conveniently within the range of one's acuteness of vision. The measurements on the additive values of the luminosity of mixed colours were made with the ordinary sectors supplied with the colour-patch apparatus.

In conclusion, I must thank those gentlemen who have participated in this discussion for their valuable and kindly criticism, which I hope may lead to the final settlement of these practically important, but debatable, points.

SOME GENERAL PRINCIPLES OF THE ABSORPTION SPECTROPHOTO-METER AND A NEW FORM OF INSTRUMENT WHICH EMBODIES THESE.

By J. R. MILNE, B.Sc., F.R.S.E., Carnegie Scholar in Physics, Edinburgh University.

THE steadily increasing importance of spectrophotometry, both for pure and for applied science, and the value of the results that might be attained, were better instrumental means at our command, have brought about the necessity for devoting time and attention to the improvement of the construction of the Spectrophotometer. The following paper contains a brief abstract of the outcome of some experimental researches by the present writer on the Spectrophotometer which, extending over a period of two years, were made primarily with a view to the construction of an instrument to give accurate results for use with considerable lengths of absorbing substance. It is intended here both to explain the general principles that have been found to apply in such cases, and also to describe a particular form of instrument devised by the author in accordance with these principles.

In the usual method of absorption spectrophotometry, the substance under examination is placed so that there passes through it one part (for example, the lower half) of a beam of light directed on to the collimator slit of the spectrophotometer, and the other part of the beam of light which does not pass through the substance is used as the standard for measurement. With such an arrangement, it is hoped that alterations in brightness of the light-source may affect equally both parts of the beam, and so cause no error in the measurements. Such a result, however, can only be expected provided that all the light of the beam comes from one and the same small area of the flame or other light-source. Again, it is in all cases desirable (and in those cases where any length of absorbing substance is in use, necessary) that *all the rays of the beam of light which traverse the absorbing substance should be strictly parallel to the axis of the beam.*—CONDITION I. The importance of this Condition may be made more apparent by taking

a specific case. Suppose we have to measure the light absorption of a solution contained in a trough 3 ft. long. A beam of light is arranged with its lower half passing through the liquid, and its upper half passing above the liquid. Now, if the beam contain rays which are not parallel to the surface of the liquid, in many cases these rays will pass partly through the liquid and partly through the air above, so to some extent defeating the object of the arrangement, which is of course to obtain one beam of light all of whose rays had passed through the whole length of the substance, and another beam of light, for comparison purposes, none of whose rays had passed through the substance to any extent whatever.

It must also be remembered that with non-parallel light the common source is virtually brought nearer if the light traverses a parallel ended substance whose refractive index is > 1, and hence, in the above case, unless parallel light were employed, the lower or "absorbed" part of the beam would be made more intense relative to the upper or "comparison" part. This source of error has been experimentally investigated by Ewan.[1]

The above considerations lead naturally to the use of a beam of light coming from a collimator which is provided at one end with an achromatic lens, and at the other with a cap having a very small hole in the line of the axis. This small hole is illuminated by projecting the image of a flat flame on to the cap by means of a projecting lens.

By using such an arrangement *three* advantages are secured: the emergent light consists of a beam of sensibly parallel rays. The light intensity across a normal cross-section of the beam is very approximately uniform. All the light leaving the collimator comes originally from a certain very small area of the flame.

In the instrument designed by the author the collimator (which has a small hole instead of the ordinary elongated slit) is mounted separately on the bed of the apparatus, and can be moved away from the prism so as to leave a space between them for the insertion of the absorbing body, whose length may be anything up to a metre.

In any form of successful spectrophotometer *some arrangement must be used to do away with the gap between the "comparison" and the "absorbed" spectra, and to bring them accurately edge to edge, because this gap greatly reduces the accuracy with which the eye can judge of the relative brightness of these spectra.* —CONDITION II. As is well known, this gap is caused by the action of the substance under examination. If the latter be a liquid, the meniscus at the ends of the trough cuts off the light of the middle part of the beam; and if it be a solid, its surface is apt to produce the same effect. In Hüfner's Rhomb, and in the author's new form of juxtapositor the rays of light are so bent that the "comparison" and the "absorbed" beams are brought into contact, and so fill up the gap between them. To obtain a successful result one condition has to be observed: before coming in contact the two beams must have their edges sharply delimited by the edges of some sort of screen, and this gives rise to a diffraction effect. After a series of experiments the author came to the conclusion that if this diffraction effect is to be unperceived by the eye, it is necessary that *the plane of the delimiting screen—whatever its particular form—should be conjugate to the retina of the observer's eye.*—CONDITION III.[2] This Condition is usually observed by placing the juxtapositor employed as close to the slit of the collimator as possible; as the latter is of course conjugate to the retina, this gives the desired result.[3] If, however, the description of collimator spoken of above is to be used in order to fulfil the first Condition, either there must be added a second collimator with the usual slit, or else an alternative position must be found for the juxtapositor which will comply with Condition III.

Instead of using either of the two juxtapositors spoken of above, however, the author made a series of experiments with a view to finding other methods of juxtaposing the two spectra. It

[1] See *Phil. Mag.* 5th Series, vol. xxxiii., April 1892, page 331.

[2] I have lately been fortunate enough to obtain a reference to a most interesting paper by E. Brodhun and O. Schönrock (*Zeitsch f. Instr.* 3 Heft. p. 70, March 1904), in which the same condition is laid down.

[3] By focussing the telescope on the diffracting edges of the juxtapositor instead of on the collimator slit, the condition can be perfectly satisfied, but only at the expense of the purity of the spectrum.

was attempted to do this by means of a bi-prism, and also, as an alternative, by means of a divided lens. The particular form of collimator already spoken of was employed, and in the case of the bi-prism the latter was placed with its refracting edge horizontal immediately behind the telescope objective, which was a cylindrical convergent lens with its axis of figure vertical. By this means the two spectra can be brought edge to edge, but the difficulty lies in complying with Condition III. The experiment of using a divided lens brought to light another necessary condition, which is one of considerable interest. A spherical convergent lens divided into two halves by a horizontal cut is placed behind the two separated spectra produced in the ordinary course by the spectrophotometer (see Fig. 1). Each half of the lens gives rise to an image of the corresponding spectrum, and by moving the
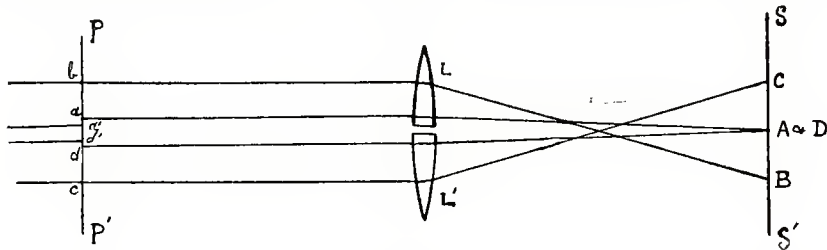

Fig. 1.

lens halves relatively to one another the two images may have their neighbouring edges brought into contact. As the plane of these images is of course conjugate to the plane in which the spectra are first formed, a screen possessing two similar rectangular apertures may be placed in the latter plane, which will sharply trim off the edges to be brought into contact, and which will at the same time comply with Condition III.

It will be found, however, that the two spectra thus formed by the action of the divided lens, when viewed by the eye, cannot be seen steadily together, but are liable to appear to move relatively to each other. The reason for this was discovered to be as follows:—The two luminous spectral strips are not like natural objects which give out rays of light in all directions from every point, but on the contrary the edge of each of the strips brought into contact gives out rays of light only in a single plane, as indicated in Fig. 2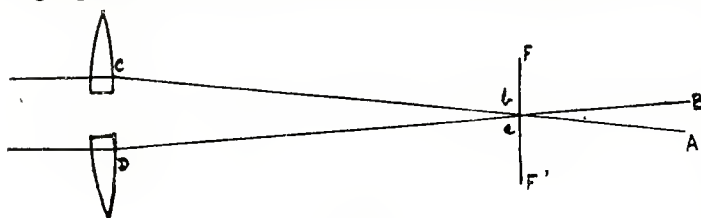. From any point a of the upper edge of the lower image rays proceed only in the plane normal to the paper which passes through the line aB, and similarly from any point b of the lower edge of the upper image the rays proceed only in the plane normal to the paper which passes through the line bA. This is due to the strictly parallel nature

Fig. 2.

of the beam of light which emerges from the collimator. Accordingly, the coincident edges of the two images are seen by means of two sets of rays which respectively fall on the optical system of the eye in planes some distance apart. Now through the effect of the eye's spherical aberration, and probably also because of general irregularities in the refractive parts of the eye, the two sets of rays from the coincident edges of the strips will not always be brought to the same line on the retina. Any slight movement of the head will alter the paths of the two sets of rays through the optical system of the eye, and the effect of such a movement will be to cause an apparent relative motion, as seen by the observer, of the really coincident edges of the two spectra.

Therefore it is necessary not only to bring accurately together the edges of the two spectra, but also (if Condition I. has been satisfied) to cause the two fans of rays which proceed from every point of the common edge to lie in one and the same plane.—CONDITION IV.

The following means of complying with this Condition was ultimately discovered. Advantage was taken of the fact that if a ray of light fall normally upon one of the faces of a Wollaston double-image prism there proceed from the other face two divergent rays which are polarised in planes at right angles to each other. If now—reversely—there fall on one of the faces of a Wollaston prism two converging rays of light inclined at the proper angle, these two rays will emerge from the opposite face of the prism in one and the same straight line normally to the face. It is true, of course, that unless the entering rays be each polarised, and polarised in the proper planes respectively, then in addition to the two coincident exit rays there will be two other non-coincident exit rays, making four exit rays in all, but the divergent rays have no connection with our present purpose, and may be disregarded, as will be shown later.

The place in the author's instrument of the Wollaston prism is shown in Fig. 3.

The eyepiece E really consists of a small astronomical telescope, which is focussed on an opaque
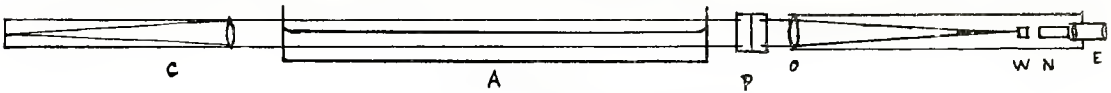

Fig. 3.

Explanation of Figure.— C—Collimator ; A—Absorption vessel containing liquid under examination ; P—Analysing Prism ; O—Ordinary Spherical Achromatic Telescope Objective ; W—Wollaston Prism ; N—Nicol Prism ; E—Eyepiece.

screen (Fig. 4) placed immediately in front of the objective O. This arrangement satisfies Condition III. This screen has two similar rectangular openings, the upper of which allows light from the upper or "comparison" beam to pass, while the lower allows light from the lower or absorbed beam to pass. Were the Wollaston removed, an observer on looking through the eyepiece would see two bright patches of light of the same colour, one above the other, separated by a short distance (see
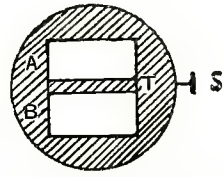
The screw S permits of adjusting the width of the "trimmer" T, separating the apertures A and B.

Fig. 4.
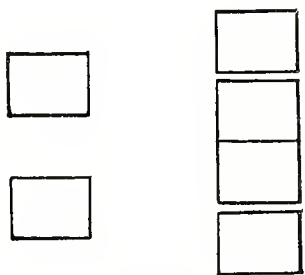
A   Fig. 5.   B

Fig. 5A). On now inserting the Wollaston the appearance shown in Fig. 5*b* is produced. The two outer patches are to be disregarded. The two in contact in the middle of the field can be brought exactly edge to edge by altering the width of the cross strip of the opaque screen, or by slightly tilting the Wollaston about an imaginary horizontal axis through the prism, normal to the optical axis of the telescope. The beams of light which form on the retina of the observer's eye the images of the two middle patches are polarised respectively in a horizontal and in a vertical direction by the Wollaston. The absorption measurements are accordingly made by rotating the Nicol N (Fig. 3) until the two patches are of equal brightness, when the tangent squared of the angle of displacement gives as usual the ratio of the brightness of the comparison and absorbed beams, and hence the light absorption of the substance placed in the instrument for the particular part of the spectrum in which the observer is working.

In judging of brightness by the eye, it is always found easier to do so in the case of a large surface than of a small one. Accordingly it may be laid down that *a spectrophotometer should present to the eye*

for comparison as to relative intensity not two narrow lines of light, but rather two broad uniformly illuminated areas.—CONDITION IV. This Condition, as appears from the foregoing (see Fig. 4), is complied with by an instrument constructed as described above.

That particular region of the spectrum which is used in any measurement is selected by means of an opaque screen with a narrow vertical slit, placed as close as possible to the Wollaston, whose cemented junction, as it lies in the principal focal plane of the lens O, is at the place where the spectrum is formed· Any required region of the spectrum can therefore be made to fall on the slit by swinging the telescope round an imaginary vertical axis through the prism P in the usual way.

It should be pointed out that, as in all polarisation spectrophotometers, a difficulty arises owing to the fact that a certain amount of polarisation is unavoidably imparted to the light by its transmission through the analysing prism. As a consequence, when no absorbing substance is placed in the instrument so that the "absorbed" and "comparison" beams are really equally intense, the Nicol when set with its principal plane at 45° to the horizontal does *not* give equality of illumination in the two patches seen by the eye. This error may be removed by inserting a $\frac{1}{4}$ λ plate between the prism P and the lens O with its axis inclined at an angle of 45° to the horizontal. The excess of light polarised vertically which passes through the prism is thus transformed into circularly polarised light, which, when resolved further on vertically and horizontally, gives rise to two equal components, instead of as before being only available exclusively in one direction. The $\frac{1}{4}$ λ plate could be built up of several strata of different materials so as to be achromatic, and to serve equally well for work in any region of the spectrum. It is simpler, however, to omit any such arrangement, and to reduce the measurements by assuming a certain hypothetical body, whose absorption for the different colours of the spectrum is to be measured beforehand once for all, to be always present in the path of one of the beams.

In connection with the optical arrangements of the instrument the following experiment is instructive : If the analysing prism P be slightly tilted so as to throw the spectrum either above or below the Wollaston prism W, the observer will nevertheless still continue to see the appearance shown in Fig. 4*b*, only now he sees but a mere outline of each patch of light, the interiors of the patches being perfectly dark. This effect is due of course to diffraction at the edges of the screen which is placed in front of the lens O, the light scattering from its edges in all directions, so that even when the prism P is tilted as above described some light from these edges still reaches the Wollaston and passes to the observer's eye. This experiment is a very pretty one and illustrates very well the chief point in the paper by Brodhun and Schönrock already referred to on page 179. In the case of the present instrument, however, the consequent difficulty found with the bi-prism arrangement dealt with in their paper would not seem to exist, or at least not in an acute form, because here the two spectra can be moved relatively to each other, and if the extreme edge of each spectrum is practically lost owing to diffraction, then the two spectra have merely to be moved rather closer together in order to bring their *apparent* edges into contact.

In designing the above type of instrument, the author had nothing in his mind but the production of a desirable spectrophotometer, and it was not until the conclusion of the experiments that the discovery was made that the apparatus was equally in effect a form of spectropolarimeter. All that requires to be done to adapt it to this end is to remove the Nicol from the eye end of the instrument, and to mount it with its graduated dial plate in front of the small collimator hole. When its principal plane is inclined at 45°[1] to the horizontal, and there is no body placed between the collimator C and the prism P, it is obvious that the two patches of light seen by the observer will appear equally bright. If an optically active body be now placed between C and P (the whole beam from top to bottom being in *this* case arranged to traverse the body), it will be necessary, if this equality of brightness is to be maintained, to undo the resulting rotation of the plane of polarisation of the light by turning the Nicol through an equal angle in the *opposite* direction.

This operation gives directly the optical rotation of the inserted substance by the reading off of the new position of the index on the Nicol dial. Furthermore, this rotation measurement is made for some

[1] Owing to the polarising effect of the prism P already discussed, the angle will not be exactly 45°.
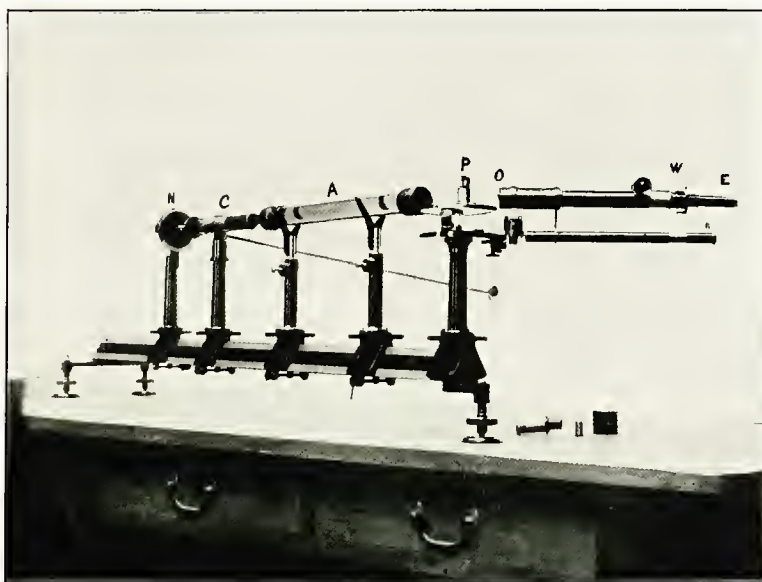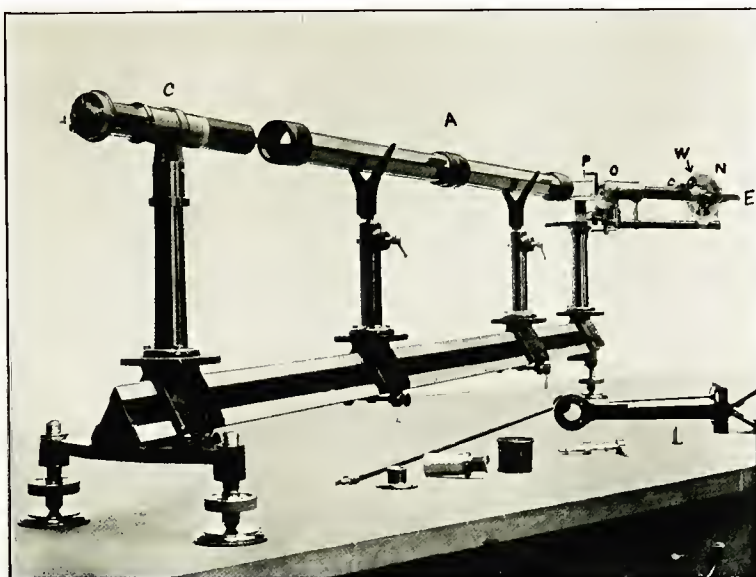
Photographs of the Instrument.

particular spectral colour of light, and the use of any other spectral colour is equally at the observer's command by merely moving the telescope slightly round. In the case of a spectropolarimeter the equal necessity for Condition I. is apparent, because if the rays of the light beam which traverse the optically active substance are not parallel, different rays will pass through different lengths of the body and hence will have their planes of polarisation rotated to different amounts. The balancing of two bright halves of the field of view as the most sensitive arrangement for polarimetry is already so well known and so widely adopted as to need no remark; but it may be pointed out that in this form of instrument a very accurate junction of the illuminated patches can be obtained, a condition which is, of course, most important for accurate work.

Summary.

To sum up, the following conditions have been found to be those which should be complied with by an absorption spectrophotometer :—

    I. All the rays of the beam of light which traverse the absorbing substance should be strictly parallel to the axis of the beam.

    II. Some arrangement must be used to do away with the gap between the "comparison" and the "absorbed" spectra, and to bring them accurately edge to edge, because this gap greatly reduces the precision with which the eye can judge of the relative brightness of these spectra.

    III. The plane of the delimiting screen—whatever its particular form—should be conjugate to the retina of the observer's eye.

    IV. It is necessary not only to bring accurately together the two spectra, but also (if Condition I. has been satisfied) to cause the two fans of rays, which proceed from every point of the common edge, to lie in one and the same plane.

    V. A spectrophotometer should present to the eye for comparison as to intensity not two narrow lines of light, but rather two broad uniformly illuminated areas.

A design of spectrophotometer is described in this paper which complies with these conditions.

This instrument possesses the further advantage that it forms a spectropolarimeter, which likewise fulfils Condition I. The eye is used under the conditions most favourable to accuracy, and a null method of measurement is employed.

Mr F. J. SELBY, M.A. (Hon. Secretary), was specially interested in the paper by Mr Milne, as he had made some experiments with an instrument similar in principle, though differing in many respects in design, viz., the König-Martens spectrophotometer. The most important difference between that instrument and the instrument Mr Milne described was that in the König-Martens instrument a finite source is used, consisting of a uniformly illuminated area, and consequently parallel light is not obtained. Had Mr Milne found any difficulty owing to reflection at the surface of the absorbing medium? There were two ways in which this might come in; from want of accuracy in the collimation, and also from want of accuracy in the setting of the surface parallel to the direction of the beam. Another point was the question of the intensity of the illumination that Mr Milne obtained in his instrument. He was using practically a point source, and one might suppose that the amount of illumination obtained would hardly be great enough to work with, especially in the extreme violet. In connection with this comes the question of the width of the slit which he uses to limit the portion of the spectrum which he actually employs in making any observation. The question of the width of slit was very important in regard to the purity of the light used; when measurements are made at a point near the maximum absorption, the variation in the amount of absorption with the wave-length is often very rapid, and if it is not possible to confine the light to quite a small portion of the spectrum, although there is no difficulty in obtaining the position of the maximum, there is great difficulty in obtaining any accurate value for the absorption co-efficient in the neighbourhood of the maximum.[1]

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[1] *Note by Author.*—In regard to such a point, see Lord Rayleigh, "Scientific Papers," Vol. I., p. 135, "On a Correction sometimes required in Curves professing to represent the Connection between Two Physical Magnitudes."

Mr CLAY said there was a difficulty in connection with Mr Milne's paper : viz., the difficulty there was with regard to using the double image method in this coloured work in connection with a prism, because the light which was transmitted by a prism was to some extent polarised. The light reflected from the first surface was polarised, and the extent of the polarisation and the proportion of the light transmitted and reflected varied from colour to colour through the spectrum, so that when one used a double image prism in which one plane is horizontal and the other plane vertical, the two beams so examined were not really truly comparable, and it was necessary to make an observation first of all without any absorbing medium in order to be able to get accurate results. The whole series of results might be entirely upset, and the absorption curve be made quite incorrect if this is not taken into account.

Mr J. R. MILNE, in reply, said there was no reason why parallel light should not be used, unless there were insurmountable objections to it later on in the apparatus. He claimed to have been able to show that there was no such insurmountable objection, and therefore one should certainly use the perfect method and not be content with a mere approximation. An acetylene flame had always given a sufficiently bright illumination to be quite measurable. Of course with violet light there was more or less difficulty, but an acetylene flame compared quite favourably with other methods of illumination. Then, as to the purity of the light with which it is possible to work, he had been accustomed to divide the spectrum into fifteen or twenty parts, but had no doubt that for special work a still smaller range of wave-length could be taken. A caution has been given as to the effect of polarisation. It did not seem to be generally known that all polarising spectrophotometers suffered from that difficulty, and that therefore, in general, a correction was always needed. In this instrument, however, the correction would not be necessary were the $\frac{1}{4}$ λ plate arrangement in use.

SATURDAY, JUNE 3.

SECTION I

Dr R. M. WALMSLEY in the Chair.

ON CERTAIN METHODS OF LENS MEASUREMENT, AND TESTING, TOGETHER WITH SOME RECOMMENDATIONS AS TO NOMEN-CLATURE AND DESCRIPTION.

By T. H. Blakesley, M.A.

I DESIRE to bring before the notice of this Convention an instrument which I have found of very varied applications in optical measurements, and at the same time to indicate the main points of theory on which these applications depend, which, though I have elsewhere dwelt upon their importance, I have had hitherto no opportunity of presenting to an assembly of this representative character.

The instrument in question is in essence a collimator, as it consists of a scale of a very few divisions in the principal focus of an achromatic lens. It may be of any size, but that which I have here, and which I have employed for some years, is small and applicable to the microscope, to the stage of which it may be fitted.

The important principles upon which the practical operations which I shall describe are based are as follows, it being supposed in all cases that the lenses dealt with are co-axial.

(1.) If light passes through two lenses whose focal lengths are $f_1 f_2$ respectively, and the distance between the second principal focus of the first lens and the first principal focus of the second lens is k, *measured positively along the axis in the direction in which the light is travelling*, then the resulting focal length of the combination is $\dfrac{f_1 f_2}{k}$.

(2.) The first principal focus of the combination is found by moving from the first principal focus of the first lens a distance $\dfrac{f_1^2}{k}$ in a direction *opposed* to the direction of light. I shall often allude to this as the travel of the first principal focus arising from the addition to the system of the second lens.

(3.) A similar motion $\dfrac{f_2}{k}$ takes place in the second principal focus, but in this case the motion, if positive, is *down* the stream of light.

(4.) If an object is in the first principal focus of a lens whose focal length is f_1, and a second lens whose focal length is f_2 is applied to the issuing light *at any distance from the first lens*, the magnification of the resulting image is constant, and its value is $\left(-\dfrac{f_2}{f_1} \right)$.

In applying these principles a thin concave lens has a *positive* focal length, a thin convex lens a *negative* focal length.

Magnification is *negative* if the image is *inverted* in relation to the object.

It will also be convenient to suppose that the light is going from right to left.

A few examples will make it clear how to deal with these principles :—

F	D	C	B		A	E
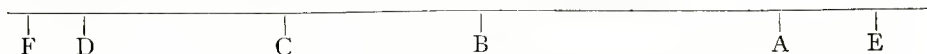
Suppose that AB are the principal foci in order of the first lens, whose focal length is -2,

And CD are the principal foci of the second lens, whose focal length is $-\frac{4}{3}$,

And that BC (or k) is $+2\cdot5$,

And that EF are the principal foci of the combination

Then the focal length of the combination $\left(\dfrac{f_1 f_2}{k}\right)$ will be $\dfrac{8}{3}\dfrac{1}{2\cdot5} = \dfrac{8}{7\cdot5} = +1\cdot\dot{0}6$.

AE, or the travel of the first principal focus, will be $\dfrac{(-2)^2}{2\cdot5} = +1\cdot6$.

DF,   „   second   „   „ $-\left(\dfrac{-4}{3}\right)^2 \dfrac{1}{2\cdot5} = +\cdot71$.

Suppose that C lies to the right hand of B by the amount of $2\cdot5$, *i.e.* $k = -2\cdot5$.

D	F	B		C	E	A

Then the focal length will be $-1\cdot\dot{0}6$.

And AE will be $-1\cdot6$.

    DF   „ $-\cdot\dot{7}1$.

If a small object, whose length measured in some agreed direction is $\cdot01$, is placed in the first principal focus of a lens whose focal length is $-2\cdot7$, and the emergent light is passed through a second lens, placed at any convenient distance from the first, whose focal length is -5, the magnification of the resulting image will be $-\dfrac{-5\cdot}{-2\cdot7}$, or $-1\cdot85\dot{1}$. It will appear inverted, therefore, and of a length equal numerically to $1\cdot85\dot{1} \times \cdot01$, or $\cdot0185$. On the other hand, were the second lens a *concave* one of focal length 5, the magnification would be $-\dfrac{5}{-2\cdot7}$, or $+1\cdot85\dot{1}$, and the image would be directed in the same way as the object, *i.e.* upright.

This matter of direction is sometimes very important for the true interpretation of the observations, and in the small scale of the collimator there should be some sign of the direction. Accordingly I employ three parallel lines at spaces taken separately and together in the ratio of 1   2   3 as ‖ |. These have been constructed by Mr J. H. Steward of the Strand, who has also made the entire instrument with very great accuracy, and though I have tested the ratios with extreme care, and repeatedly, I can find no error in them. With such an object one is able at a glance to say that the magnification is positive or negative, as the case may be. The advantage of the simple ratios mentioned above is the following. The whole width being the one I usually employ, the observations made consist in counting the number of divisions and the fraction over, which its image occupies in the scale which is in the eyepiece of the microscope. Now it may happen that the image extends beyond the limits of this scale. If it does so one employs at once the image of the smaller or larger portion, and multiplies the number of divisions occupied by it by 3 or $\frac{3}{2}$, as the case may be, employing the result with confidence, as if it had been directly the subject of observation.

The microscope employed is furnished with a scale and vernier to measure the motion of the microscope longitudinally, relatively to the fixed portion of its stand.

In order to standardise the instrument for practical use it is necessary—

(1) To separate the lens from the instrument, and to observe the number of divisions of the microscope scale occupied by the image when the object is viewed directly through the

miscroscope. In my instrument this number is 24·60 when the usual objective is in employment. For other objectives there are other numbers which have been duly registered.

(2) To find the focal length of the collimator lens. Its value in this instrument is – 2·746 cm. which has been determined by measuring the change in the magnification produced by introducing accurately measured distance tubes between the object and the lens, in the way I have described elsewhere.[1]

(3) To adjust the object in the principal focus of the lens. To effect this the lens and object are carried by two tubes sliding stiffly one in the other.

The usual reflecting condenser is replaced for the time by a piece of plane speculum, and the collimator is placed upon the stage with the lens downwards towards the speculum, and the object upwards. An image of the moon near the quarters is then by means of light reflected from the speculum and passed through the lens, formed at the principal focus. The lines of the object are then brought to coincidence with this image by operating the sliding tubes. The adjustment is complete when the lunar surface and the lines are seen clearly at the same time. This may be effected on any microscope.

As a measure of precaution, in case of the instrument being put out of adjustment, I unscrew the lens and gauge with the callipers the length of the instrument. The length here is 2·805 cm. The lens is removed only to avoid scratching the surface with the callipers.

When in use the collimator is placed through the hole in the stage, upon which it rests by means of a collar near the lens, the lens itself being turned towards the microscope. When another lens is placed between the collimator and the microscope, an image of the object will be formed in the second principal focus of the second lens, whose size relatively to the object will be $\left(-\dfrac{f_2}{f_1} \right) : 1$.

This image when viewed (as an object) in the microscope will produce an image at the scale in the eyepiece occupying a number of divisions which can be counted. This number by principle (4) will bear to the number observed when the object was viewed directly (24·60) the ratio which the focal length of the inserted lens bears to the focal length of the collimator lens, with the negative sign applied. With this instrument, for instance, if n is the number of divisions occupied by the image when the second lens of unknown focal length F is placed between the collimator and the microscope

$$\frac{n}{24\cdot60} = \frac{-\,F}{-\,2\cdot746} = \frac{F}{2\cdot746}$$

$$\text{Hence } F = \frac{2\cdot764}{24\cdot60}\, n = \cdot1116n.$$

If the image is inverted, n is to be taken as negative and F will have a negative value, indicating a convex character in the lens.

The number ·1116 obtained as I have described is the constant of the arrangement.

To employ the instrument to find focal lengths, multiply the constant by the number of divisions observed.

It is unnecessary to enter deeply into an argument as to which shall be considered the direct or inverted aspect. Generally the character of a lens is obvious on other grounds, but the sign of the aspect may be at once fixed by placing a small double convex lens in position, as if to be tested, and observing the aspect it presents, which is then to be held as the negative one.

The positive focal lengths of concave lenses present no greater difficulty than lies in the fact that often the second principal focus is behind the lens (on that side of it from which the light is coming). It is necessary that the microscope shall be able to bring this point into focus, and therefore very powerful microscope objectives might be inapplicable. It is only necessary that the working distance of the microscope shall be as great as the distance of the principal focus behind the upper surface of the concave lens.

In some cases even this difficulty does not exist. For instance, a microscope is itself essentially

[1] " Geometrical Optics." London, Whittaker & Co., 2 White Hart Street, Paternoster Square, and 66 Fifth Avenue, New York.

a concave arrangement, and has a positive focal length, but its first principal focus lies *in front of* the first surface and its second principal focus *beyond* the second surface. I have found the method directly applicable to an entire microscope, the tube of which was not too long.

To find the focal distance defined as the distance of the principal focus from the surface of the lens system.

After focussing as if to find the focal length, read the longitudinal scale and vernier. Then focus upon the nearest surface of the lens, which may be easily identified by putting upon it a little lycopodium dust, and read the scale and vernier again. The difference of the two readings readily gives the value of the quantity desired, and the information whether the principal focus lies within or without the system, and by how much; in other words, it gives the distance of the principal point from the surface.

The same quantities can be obtained for the other principal focus, surface, and principal point.

In a single-piece lens, to find the ratio of the radii of the surfaces.—This ratio is equal to that of the distances of the principal points from the surfaces corresponding.

To find the distance between the *second* principal focus of one lens and the first principal focus of the second lens in a combination of two lenses.

Find the focal lengths successively by the method described of each lens separately, and of the combination. Let these be f_1, f_2, F respectively. Then the distance required is $\dfrac{f_1 f_2}{F}$.

This operation is of great importance in the case of eyepieces which are apparently wanting in achromatism. Take Huyghens' eyepiece as an example.

The definition of a properly arranged Huyghens' eyepiece contains the conditions that

$$f_1 : f_2 : k : : -1 \quad -3 : -2.$$

The measurements of f_1 and f_2 give at once the relation between the focal lengths, and settle how far the condition $f_1 : f_2 : : -1 : -3$ is satisfied, and the measurement of F as well fixes the relative position of the two lenses.

Now whether the relation $-1 : -3$ is exactly carried out is not a matter of extreme importance, as is the particular relation in position which makes the combination achromatic. The condition for this is that with lenses of the same glass $k = \dfrac{f_1 + f_2}{2}$, which is the value which k ought to possess. In any case it does possess the value $\dfrac{f_1 f_2}{F}$, and if these differ, the tube ought to be lengthened by the value of the difference between

$$\frac{f_1 + f_2}{2} \quad \text{and} \quad \frac{f_1 f_2}{F}.$$

It is necessary to be careful in dealing with the signs here. If for convex lenses the focal length is taken as negative. The lengthening of the tube between the lenses necessary to make the combination achromatic is

$$\left\{ \frac{f_1 + f_2}{2} - \frac{f_1 f_2}{F} \right\}$$

and if this number has a negative value a shortening of the tube is necessary.

All the other combinations which have a fixed definition, as Wollaston's doublet, in which

$$f_1 : f_2 : k : : -2 : -6 : -5$$

or as Ramsden's eyepiece in which

$$f_1 : f_2 : k : : -3 : -3 : -4$$

may be equally easily tested by the microscope collimator, and all other combinations which are not defined may have their arrangements analysed by the instrument.

To find the Curvature of a Lens Surface.—Between the collimator and the lens to be measured place a plate of optically true glass, and lay the lens on the surface, with the face to be measured upwards.

Find its *focal length* f, and the *focal distance* u_0 from the upper surface.

Reverse the lens upon the plate, and place a drop of water between the plate and the lens. Find

the focal length F of the combination of lenses made up of the lens under experiment and the water lens between the two surfaces. Then the radius of curvature required (r) is given by

$$r = \frac{\overline{\mu - 1} \cdot u_o}{1 - \dfrac{f}{F}}$$

where μ is the index for water, and if r has a positive value, the surface is to be interpreted as convex. $\overline{\mu - 1}$ may be taken as equal to ·3336. Similarly, the curvature of the other surface may be found.

This method is not restricted to simple lenses nor to convex surfaces.

To find the Index of Refraction of a Liquid.—When a lens has been accurately measured as to its *curvatures, focal length, and focal distances*, and may be considered as standardised, it may be used to measure the index of any liquid placed as the water was in the preceding operation, by applying the same formula for determining $\overline{\mu - 1}$, when μ applies to the liquid in question ; or :

$$\overline{\mu - 1} = \left\{ 1 - \frac{f}{F} \right\} \frac{r}{u_{,,}}.$$

The only fresh measurement necessary is F, the focal length of the combination.

In all the above-mentioned methods the results are mathematically exact—that is to say, the thicknesses of the lenses have been in no way neglected, and any errors that arise are due solely to those of observation, and may be removed by repeated observations and variation of constants, as in all scientific determinations. For instance, more than one collimator may be employed and more than one objective in the microscope. The focal length of the collimator lens should indeed increase with that of the lenses to be examined, and when the scale is sufficiently large, as in the case of telescope objectives, the microscope may be dispensed with, and an optical bank substituted for it, carrying a simple glass scale to be examined with an ordinary hand lens.

The spherical aberrations for rays parallel or oblique to the axis, and any chromatic aberration, may be investigated with such a collimator as I have described of whatever scale, if the collimator lens is trustworthy in these respects.

For the chromatic aberrations it is only necessary to employ light of various colours derived from a good spectroscope to see if the focal lengths, etc., remain the same in all cases.

Spherical aberration may be detected by moving the collimator always parallel to itself to the marginal portions of the lens which it is intended to employ with parallel light, as for instance a telescope objective, carefully constructed moving apparatus being of course required for the operation. Where no spherical aberration exists no motion should take place in the image under these circumstances, whatever the angle between the axis of lens and collimator. In microscopic objectives the light in this case should be passed through the lens in the opposite direction to that which passes in the ordinary use of the instrument. If in such motions the size of the image remains always the same, distortion is eliminated, and finally, if the image remains in focus, flatness of field has been secured.

I wish here particularly to point out and emphasise the fact, which I believe has hitherto been neither recognised nor applied to the purposes of measurement, that the four propositions upon which I have laid stress at the beginning of this paper *have exact counterparts when we are dealing with oblique pencils.* Hence along any oblique ray may be considered to exist a scale of magnification in which successive numbers are separated by *fixed* distances (focal lengths), which can be dealt with *exactly as those along the axis of the system.* The focal lengths on oblique lines can be determined by one observation with a collimator, and the focal distances may similarly be determined. If a lens system be viewed in this way—and I hope that Mr Chalmers and Dr Drysdale, who have given so much time and skill to the study of aberrations, will do me the favour to consider this view—I think the whole system of aberrations may be greatly simplified. For instance, in an ideally perfect system the focal surface of a plane object should be a plane image in all cases. This would imply that the focal length f' along any line making an angle θ with the axis multiplied by the cosine of that angle should equal the focal length

f. If any difference be found to exist between f' and $\dfrac{f}{\cos\theta}$ it might receive an appropriate name, and be treated and measured as an aberration in f'.

Then, again, f' and $\dfrac{f}{\cos\theta}$ might be equal, without the magnification points coinciding with the planes through the corresponding points along the main axis. Here would be a constant aberration in position along the line considered.

Just as the difference in position of two points—say two stars—may be variously stated in right ascension and declination, in longitude and latitude, or in altitude and azimuth, according to convenience, so the departure of a focus from its ideal position may be stated in aberrations in different ways; and it seems to me that the view I have here put forward is a simpler one than those involved in many of the classifications at present in vogue, and that it possesses advantages not merely on that account, but also in respect of the ease with which the quantities concerned can be measured in practice. I cannot pretend to exhaust this subject here.

I would conclude this paper by respectfully suggesting that a little more exactitude might be used in the description of instruments.

The focal length is the principal requirement in lenses which are otherwise well constructed, and should be given at least to three significant digits, in all lenses whatever.

Focal distances on both sides should be given to the same degree of exactitude in all photographic lenses, and the external focal distance at any rate in all microscopic objectives.

The focal length of a microscope, as a whole, might also be stated—that is to say, the focal length for each combination of objective and eyepiece, the instrument being at its shortest, and if required at any specified elongation. The rules I have given enable this to be done easily and quickly.

A stricter distinction should be maintained between the terms "focal length," "focal distance," and even "focus," which are too often employed for the same thing. When it is difficult to find words to express the various ideas which have to be dealt with, it is worse than harmless to expend two or three upon the same idea.

In the case of telescope objectives, I would suggest in dealing with spherical aberration that the expression *marginal intercept* should be used to denote the length of line from the point where a marginal ray at length cuts the axis, backwards to its intersection with the original course of the same ray before impact on the lens.

It would have to be followed by some expression denoting the course of the ray as being a certain distance from the axis originally, or as finally making a certain angle with the axis, so that such expressions would be employed as—

Marginal intercept for two inches, or
Marginal intercept for 2°30′

according to the view taken.

Finally, I should be glad to see the decimal system and the centimetre unit of measurement universally employed.

The CHAIRMAN (Dr WALMSLEY) expressed his gratification that this method of treating optical problems had been brought before the Convention. He did not wish to discuss the paper, but would be glad if Mr Blakesley, in his reply, could give the reasons for departing from the usual notation as regards signs. Uniformity in such matters was of very great importance, and he trusted that an expression of opinion by the members present would tend to secure this uniformity.

Mr C. BECK was much interested in the paper and in the apparatus described by Mr Blakesley, which ought to be of considerable value in testing short focus negative lenses. Mr Beck described his own method of measuring short focus positive lenses. A microscope with extra adjustments and means of measuring the movements of the sub-stage is used. The lens to be tested is placed on the stage, and a micrometer in the sub-stage; the latter is racked up or down, and the microscope focussed, till the

image formed by the lens is of exactly the same size as the micrometer; the focus of the microscope objective then coincides with one of the symmetrical planes. To determine the principal focus a plane mirror is used to reflect light from a distant object through the test lens and the microscope focussed on the image so formed. The focal length of the test lens is read off directly as the distance moved by the microscope between the two readings. The distance of the focus from the lens surface can also be measured directly.

This method was, of course, inapplicable to negative lenses, and was difficult to apply with sufficient accuracy to uncorrected lenses. In the latter case Mr Beck preferred to measure the curves of the lens. He had tried a method similar to that which Dr Drysdale had recently so much improved; he had placed his mirror in front of the objective, reflected a parallel beam of light on to the test surface, and measured the distance between the surface and the final focus. The method was suitable for measuring curves down to $\frac{5}{100}$ inch radius, but the accurate spherometer they now used was suitable for radii of $\frac{6}{100}$ or $\frac{7}{100}''$, and was more convenient.

As regards the question of signs, Mr Beck thought that the direction in which the light is travelling should always be taken positive, and lenses which produce convergence should be regarded as positive. With reference to the marginal intercept referred to by Mr Blakesley, he thought the most convenient way of expressing the aberration was that described by Steinheil and Voit, in which the angle of inclination to the axis of a ray was considered, and its departure from its theoretical amount became the measure of this aberration. It was evident that a ray could have an error either in its focussing position or in its inclination to the axis, and the latter error might remain, even if the first were corrected.

Mr SELBY wished to emphasise the usefulness of the method of expressing conjugate distances and the simplicity of the formulæ. In Mr Blakesley's notation $pq = f^2$ replaced $\frac{1}{v} - \frac{1}{u} = \frac{1}{f}$, and the formula was specially convenient for allowing for a change in the object distance. The magnification was expressed as $\frac{f}{p}$ or $\frac{q}{f}$, and any change in the magnification was directly proportional to the change in q, a statement which was most convenient as the basis of all measurements by which focal lengths were obtained by measurement of two magnifications.

Dr DRYSDALE endorsed Mr Selby's remarks as to the convenience of Mr Blakesley's notation for certain calculations. As regards the question of signs, he thought the most convenient method was to regard all curvatures as positive when the surfaces were convex to the incident light, and curvatures of waves to be positive when the light was converging. In the case of mirrors this did not make all lines in one direction positive, as the distance would be reckoned positive if measured in the direction in which the light is actually travelling.

With regard to curvature measurement, he thought Mr Blakesley's method extremely pretty, but preferred a more direct method—the auto-collimation method, or possibly the spherometer. He had not had the opportunity of examining Mr Beck's spherometer, and was surprised to hear that it was capable of measuring such small lenses. The auto-collimation method was, he thought, specially suitable for small lenses. He could not see how Mr Blakesley's idea of measuring oblique focal lengths could be carried out, as the so-called focal lengths would seem to vary from place to place.

Professor S. P. THOMPSON said that Mr Blakesley's work was characterised by some new and useful ideas; the principle that the magnification was altered by unity when the image was displaced a distance equal to the focal length was invaluable. It was, of course, an extension of well-known principles, but in its new form was specially convenient.

With regard to the question of signs, Professor Thompson agreed with Dr Drysdale's practice; he called a lens positive if it produced a convergence. He always considered the light travelling from left to right, and thus positive directions were measured in the direction the light was travelling, and a real image produced where the abscissa was positive. The most convenient origin for the co-ordinates was

undoubtedly the second principal plane, as this enables us to obtain the distances from the principal focus also.

Mr CHALMERS requested that, as far as possible, users of other systems might respect the convention of users of the curvature method, that small letters should represent distances, and capitals the corresponding curvatures. He thought that Mr Blakesley's formula $\dfrac{f_1 f_2}{k} = f$ was even more useful when it was expressed in terms of curvatures $F = kF_1F_2$. He preferred to use it in the form $F = -kF_1F_2$, not to depart from the usual practice of calling a converging lens positive. The importance of expressing results in terms of the magnification could hardly be over-estimated, and especially was this the case in dealing with aberrations.

Mr BLAKESLEY, in reply, would not insist on his notation as regards positive and negative focal lengths, but considered it absolutely essential that a magnification should be called minus when the image was reversed. Dr Drysdale had touched on the question of measuring focal lengths. He (Mr Blakesley) had every reason to believe that for oblique pencils the magnifications increased by unity for equal changes in the distance of the image plane, although these distances would not be the same as in the case of axial pencils; a certain reduction would be necessary before the distances could be strictly compared, and their differences made a measure of the aberration.

POSSIBLE DIRECTIONS OF PROGRESS IN OPTICAL GLASS.

By WALTER ROSENHAIN, B.A., B.C.E.

THE epoch-making work of Schott and Abbé, and its developments at the Jena works, have made and are still making their mark in every field of optics; but while fully valuing this work and the results which have flowed from it, the writer is inclined to deplore one consequence to which it seems to have led—namely, a species of contentment with results already achieved, and a concomitant slackening of the demand for further advance. The progress in optical glass during the past twenty-five years has not, in all probability, exhausted the possibilities of homogeneous transparent media which might be made available to the optician. Quite recently we have witnessed the introduction of microscope lenses made of fused silica, and of glasses made particularly transparent to ultra-violet light, while the use of the natural mineral fluorite in certain microscope objectives illustrates the fact that the glass-maker has not as yet met all the desiderata of the optician. The probability is, however, that in this field all the easier tasks have been accomplished, and that thus the fairly obvious commercial possibilities of optical glass have been exploited. If this be so, further progress must depend upon investigations made for scientific purposes and not undertaken commercially. It is, of course, more than likely that commercial success might ultimately reward an investigation of that kind, but the probabilities of such an issue are not sufficiently clear to encourage anyone to embark on such a task on purely business principles. It is interesting to recall the fact that the great development of optical glass, which has originated at Jena, began as a purely scientific research, rendered possible by the timely financial aid of the Prussian Government. It is perhaps permissible to express the hope that if on further consideration the investigations outlined in the present paper are found sufficiently promising to be put into practice, this forward step may not be left to be taken by workers in other countries.

Leaving aside for the time all questions of ways and means, the present paper is intended to point out some possible directions in which transparent media, widely different from optical glasses as now available, might be sought. For obvious reasons, the author proposes to deal with the question rather from the physico-chemical standpoint of the modes of production of such media than from the point of

view of their most useful application; he is, however, sufficiently acquainted with the requirements of opticians to be aware that a wide extension of the available range of optical materials would be warmly welcomed, and would be likely to lead to great advances in optical systems. It should perhaps be explained that the term "transparent media" has been designedly used in preference to the more familiar word "glass," because the author believes that, at all events in certain cases, the substances in question can only be obtained in the crystalline state, to which the term "glass" cannot be legitimately applied.

On looking at the lists of optical glasses now available, one is struck by the fact that the limits between which the chief optical constants vary throughout the entire range of glasses are distinctly narrow. The refractive index always lies between 1·46 and 1·90, while the constant generally designated by the symbol v varies from 67 to 29, and in estimating this range glasses are included which are not well suited to most practical uses. The question naturally arises whether these limits are purely accidental or whether there is some physical fact at their basis. In favour of the latter view considerable evidence is to be found, principally in the nature and behaviour of the glasses which form the extreme members of the existing series, and particularly of those in which the relation of dispersion to refraction is abnormal, *i.e.* in which a high refractive index is combined with a low dispersion. In order to appreciate this evidence, and also with a view to leading up to ideas required in a later part of the present paper, it will be desirable to briefly consider the physico-chemical conditions governing the production of a glass.

In the fused state a glass may be regarded as a homogeneous solution of a number of chemical compounds; in solidifying when cooling, such a solution may behave, so far as the final result is concerned, in either of two ways: in one case solidification begins sharply at some definite temperature dependent only on the chemical composition of the mass, and the absolute pressure, and the resulting solid mass, when cooling has been carried far enough for complete solidification, forms an aggregate of crystals; in the other case, cooling continues without the occurrence of a definite change at any definite temperature, the liquid gradually becomes more and more viscous, until at a sufficiently low temperature the viscosity is so great that the mass has most of the properties of a solid, and crystallisation is permanently inhibited. It is probably a sound generalisation to say that, under suitable conditions, any fluid could be made to undergo either of these processes, *i.e.* could be caused to solidify in the vitreous, amorphous state, or in the crystalline state.[1]

The conditions which govern the mode of solidification resulting in any particular case vary considerably with the chemical nature of the substances in question; as in our ordinary laboratory and industrial operations the rates of cooling do not vary very widely, it is ordinarily stated that some substances, such as silicates, naturally assume the vitreous state, while metals habitually assume the crystalline state; but—theoretically, at all events—this is merely a question of the rate of cooling and of the absolute pressure. As a matter of fact, in the case of 153 substances chosen at random by Tammann, 59 could be obtained in the vitreous state, while only 22 could not be seriously undercooled; on the other hand, every known vitreous substance, with the exception of boric anhydride, can be caused to crystallise by suitable treatment.

Taking, then, the conclusion that glasses are simply greatly under-cooled liquids produced by a rate of cooling sufficiently rapid to prevent crystallisation from setting in during the passage through the critical range of temperature, the behaviour of extreme glasses in this respect may be considered. It is a statement amply justified by experience, that when the chemical composition of a glass flux is altered with a view to obtaining extreme optical properties, the tendency towards crystallisation increase rapidly. Up to a certain point this can be and is overcome by accelerating the cooling of the glass, but it cannot be carried further without breaking the mass down into useless fragments, and this forms one of the limits to the range of optical glasses in several directions. Possible means of overcoming this

[1] For some of the facts and many of the theoretical deductions contained in this section, the author is indebted to Prof. G. Tammann, whose book "Kristallisieren und Schmelzen" (Barth., Leipzig, 1903) is an exhaustive treatise on these questions.

difficulty are, indeed, suggested by one of Tammann's conclusions. He has shown that, for certain chemically homogeneous bodies, the melting-point is raised by raising the absolute pressure, ultimately reaching a maximum value. It follows that a more rapid passage through the critical range would be possible when the position of that range on the temperature scale was considerably raised. Consequently glasses which show too great a tendency to crystallise under ordinary conditions might be retained in the vitreous state by cooling rapidly under pressure. The pressures required are, however, enormous, and under existing conditions the application of this interesting theoretical conclusion to practice does not seem possible.

Returning to the natural limitations in the production of optical glass, it is a further fact of some importance that all extreme glasses are of the nature of very active chemical agents, both in the fused state and in the ordinary solid condition. The latter fact has eliminated from practice a large number of glasses of most desirable optical properties, as they proved entirely unstable in contact with the atmosphere ; while the former condition imposes a limit on the production of such glasses on account of the destruction of the vessels in which the glass is melted. It is, however, a curious fact that the tendency of such extreme glasses in the fused state is to take up from surrounding bodies such substances as will tend to change the optical properties of the glass *towards*, and not away from, the "normal" condition.

The conclusion to which the author is thus inclined to come is that the range of optical glasses is limited by physical and chemical conditions which are intimately connected with the optical effects of the glasses, the most serious of these limiting causes being the tendency to crystallisation. On Drude's electro-magnetic theory of refraction and dispersion, the optical constants of a glass depend upon the position of its absorption bands in the ultra-violet or the infra-red region of the spectrum, and it is possible that the constitution of molecules, or groupings of molecules, requisite to effect the absorptions correlated to "abnormal" optical constants, is such as cannot readily exist without the additional support or stability derived from crystalline arrangements.

From the considerations here advanced, the author is inclined to draw the conclusion that any considerable extension of the range of available optical glasses is not likely to be made on lines at all analogous to those pursued in the production of glasses. The most promising direction of progress is to be found, in his opinion, by accepting the limitations discussed above, and in fact taking the line of advance indicated by the most serious of those limitations, viz., the tendency to crystallisation. The object to be aimed at, then, becomes the production of crystals of composition and properties suitable for optical uses. It must, of course, be admitted at the outset that the task is an exceedingly difficult one, but probably not more so than the problem of the production of homogeneous optical glass in large masses must have appeared to the men who attacked that problem a century ago. Before, however, dealing with some of these difficulties in detail, it will be well to consider some of the definitely known factors of the question.

In those cases which have so far been measured, a considerable difference has always been found in the optical behaviour of the same chemical substance in the vitreous and the crystalline condition. This is well known in the case of silica, and in the case of several experimental glasses produced by the author, of chemical composition identical with that of certain minerals, the same fact was met with, but to a still more marked degree. In these latter cases, the optical behaviour of the crystalline mineral was very "abnormal," and here again the change produced by the conversion into the vitreous state was in the direction of more normal optical properties. The author has also studied the crystals formed during the slow cooling of some optical glasses of extreme properties, and so far as microscopic methods would permit of their determination, these crystals also differed widely in optical behaviour from the glass in which they had been formed. These facts are referred to here because the author wishes to emphasise the fact that he is not advocating the attempt to produce novel glasses by the imitation of the composition of minerals of promising optical properties, nor yet the attempt to obtain in optically useful form the crystallisations resulting from the "devitrification" of extreme optical glasses, as he does not

regard either of these processes as particularly promising. The course that is most likely to lead to valuable results is arrived at from considerations of the conditions to be fulfilled by a crystalline material that is to be used for optical purposes.

The first of these conditions is, of course, transparency, and the exclusion of all colouring oxides thus becomes imperative; the great majority of natural minerals are ruled out by this condition. The condition of perfect transparency also demands that individual crystals of sufficient size can alone be used, crystalline aggregates being useless; for anything beyond the smaller microscope lenses, therefore, the production of large and perfect artificial crystals would be essential. Also, where the optical system is intended for the transmission of rays at a considerable angle to the axis, double refraction in crystals would be a fatal objection, and this condition restricts the available materials to those which crystallise in the regular system. Finally, the optical properties of the artificial crystals sought must be of special value to the optician.

The only possible preliminary guidance in the study of this question is to be derived from a knowledge of the optical properties of natural crystals of the regular system. Unfortunately, the optical properties of most of these have only been studied hitherto with a view to their identification by the mineralogist. A preliminary to the course of investigation here suggested would therefore consist in the detailed investigation of the optical properties of natural minerals, from the optician's point of view, and for a reason to be presently given, the coloured minerals should not be excluded from this study. Even this preliminary investigation is beset with difficulties, owing to the fact that many mineral varieties could only be obtained in very small pieces. Modern optical appliances should, however, overcome this difficulty. As a preliminary indication of the range of optical properties likely to be met with, the following table of refractive indices of minerals crystallising in the regular system is given, being taken from Rosenbusch's " Hilfstabellen zur Mineralbestimmung."

TABLE OF NATURAL MINERALS CRYSTALLISING IN THE REGULAR SYSTEM.

Name of Mineral.	Optical Properties.		Empirical Chemical Formula.
	$^n\mathrm{D}$	ν	
Opal	1·45	..	$SiO_2 \cdot xAq$
Spinells—			
Spinell	1·715	...	$MgAl_2O_4$
Hercynite	1·749	...	$FeAl_2O_4$
Gahnite	1·765	...	$ZnAl_2O_4$
Chromite	2·096	...	$FeCr_2O_4$
Fluorite	1·4338	95·4	CaF_2
Garnets	1·747 to	...	(Ca,Fe,Mn,Mg)
	1·812		$(Al,Fe,Cr)_2 Si_3^3 O_{12}$
Leucite	1·508	...	$(K,Na)_2 Al_2 Si_4 O_{12}$
Sodalite	1·484	...	$2(Na_2Al_2Si_2O_8) + NaCl$
Hauyn	1·496	...	$2(CaAl_2Si_2O_8)$
Zeolite	1·488	...	$Na_2Al_2Si_4O_{12} + 2Aq$
Perowskite	2·38	...	$CaTiO_2$

(From H. Rosenbusch, *Hulfstabellen zur Mikroskopischen Mineralbe stimmung in Gesteinen.* Stuttgart, E. Koch, 1888.)

It will be seen from this table that a very considerable extension of optical properties would be made available by the artificial production of these minerals in an optically useful form, but it must be remembered that the minerals shown in this list do not represent the available extremes, as a large number of crystalline bodies are known in the laboratory of the inorganic chemist whose properties of hardness and permanence would fully equal those of the natural species, while their artificial production would probably present no greater difficulty. Further, our knowledge of the nature of crystals enables one to foresee that in all probability a very great range of intermediate species, and even of more extreme

varieties, might be produced, principally by two well-known methods of changing the properties of crystals—viz., by the substitution of one chemical element for another of the same group, either wholly or in part, and by the production of mixed crystals or solid solutions. In connection with the former mode of procedure, it would be interesting, for example, to obtain a fluorite in which the calcium had been replaced by magnesium, or by strontium or barium, substitutions which would in all probability be readily effected once the method of artificially producing fluorite crystals had been mastered. Colourless analogues of coloured minerals might also be obtained in this way.

With these indications of the possibilities of the field, it remains to consider what is our existing knowledge of the artificial production of mineral crystals, and especially of large crystals. It is of course well known that a great number of natural minerals have been produced artificially, but so far as the author is aware, considerable gaps remain to be filled up ; that these minerals have not as yet been produced in large crystals is also well known—otherwise the world would long have been flooded with artificial precious stones. On the other hand, recent advances in physical chemistry have greatly extended our knowledge of the mode of formation of crystals, and have elucidated some of the conditions which govern their rate of growth and the size attained. In a general way, it has long been known that gradual solidification, either from the molten or dissolved state, is favourable to the formation of large crystals, but the work of Tammann has thrown further light on this subject. He has studied the formation of centres of crystallisation in variously under-cooled liquids, by exposing the liquid in question to a certain degree of under-cooling for a measured time. A certain number of centres of crystallisation are formed under these conditions, but if the under-cooling is sufficient they remain entirely invisible for a very long time as, owing to the diminution of the rate of crystalline growth with fall of temperature, the rate of crystallisation is too slow : this rate is, however, readily accelerated by raising the liquid to a temperature just below its normal freezing point, and the previously invisible centres of crystallisation are then developed into small crystals, and may be counted. This process is interesting in the present connection because it suggests a means, in certain cases, of regulating the number of centres of crystallisation formed in a given mass of liquid by suitably timing its exposure to a low temperature ; if only a very few such centres be allowed to form, and the liquid then reheated to a suitable temperature and maintained there, comparatively rapid crystallisation might be allowed to take place compatibly with the production of large individuals. It should perhaps be recalled in this connection that the matrices from which crystallisation takes place are frequently solid, and that, no doubt, the above order of ideas is equally applicable to these. Perhaps the most important inference in the present connection from these results of modern physico-chemical research is that extremely slow cooling through very wide ranges of temperature is not, in many cases, essential to the production of large crystals, but that the rate of passing through certain easily ascertained critical temperatures is the governing condition ; this renders the whole problem much more hopeful of practical solution, since modern electrical methods render the accurate measurement and control of high temperatures possible. The natural process of mineral formation has, no doubt, as a rule, involved extremely slow cooling through the entire range, but it seems that for practical purposes it will only be necessary to imitate a very small part of this process, so that hours instead of centuries may suffice.

Another large class of crystalline bodies is formed without the intervention of fusion, and the mode of production of large crystals from æqueous solutions is already well known where their production depends on deposition from ordinary solution. Further possibilities are presented by the gradual formation of crystalline substances insoluble in the liquid present, by gradual chemical action between dissolved bodies. Crystalline bodies may also be formed by deposition from the gaseous state, and the influence of temperature, pressure, and ionisation on these actions remains to be studied.

The object of the present paper being to indicate the possibilities of progress, it would not be profitable to pursue these questions any farther. The author believes, however, that enough has been said to show that in the study of the nature and mode of production of large mineral crystals may well lie the key to further progress in optical materials.

Dr DRYSDALE congratulated Mr Rosenhain on a paper which was bound to have a far-reaching influence; much had been written on optical glass, much progress had been made and wonderful work accomplished at Jena, but this paper indicated possibilities of even greater progress.

As a designer of lenses he appreciated the necessity for a wider range of optical glasses, and could assure Mr Rosenhain that any progress in the direction of providing a wider range in the values of the refractive index and dispersion would have an immediate practical application. In the design of binocular objectives higher aperture ratios were desirable, but with the present range in the values of v were unattainable.

He was pleased to see that Mr Rosenhain had made such a strikingly new departure in attacking this problem, especially as the present lines of advance seemed to have almost reached their limits.

Mr C. BECK said that all opticians in this country had watched with great interest the enormous improvements in optical glass, made first of all in Jena and more recently at Birmingham, through the direct influence of Mr Rosenhain.

Mr Rosenhain had been so successful in the manufacture of optical glass that any suggestion he had put forward was worthy of every consideration, and his efforts in the direction of manufacturing crystals suitable for optical work deserved the support of all interested in the industry.

Prof. SILVANUS P. THOMPSON said the paper had been specially interesting to him, and was one of the most valuable contributions to the proceedings of the Optical Convention. It showed that there were possibilities of progress of which most members had never dreamed.

Although it might be true that it would not pay any glassmaker to make the expensive researches necessary, he thought the idea sufficiently promising to induce some optical firms to take the matter up, and that the Royal Society might be appealed to for a grant in aid of the research.

He pointed out that it would be difficult to obtain the minerals mentioned perfectly free from double refraction, although they were nominally singly refracting; he had never come across a diamond of any size, or a piece of alum that did not show some trace of double refraction. Brewster had investigated the matter, giving to the phenomena the name of Lamellar Polarisation. It might be that these imperfections were due to the process of manufacture, to strain during the process of growth or cooling, but they would have to be taken into account in any attempt to manufacture crystals of large sizes.

The growth of crystals was largely influenced by the material in which they were formed; it was not, however, necessary, as he understood Mr Rosenhain to suggest, that each substance should be crystallised out from its own fusion or its own solution, as the case might be. It was possible to have foreign substances in a solution, which are not deposited in the crystal, but, nevertheless, influence the state and form assumed by the crystal. For example, alum crystallised out from water in the form of octohedra, but if alcohol were added to the water the crystals would be regular cubes. The garnet—to take a mineral cited by Mr Rosenhain—might occur in two forms. Common salt only crystallized out from aqueous solutions in cubes, sometimes in cubes with hollow faces like bismuth crystals, but if urea were present the salt came down as octohedra.

These illustrations showed that the nature of the medium in which the crystals are formed had a very great influence on the ultimate form of the crystal, and any research on this point would have a number of curious and anomalous points to clear up.

He (Prof. Thompson) noticed that Mr Rosenhain was very hopeful of having mastered the delicate question of maintaining the temperature constant within a narrow range, and, thanks mainly to the work of Callendar and Griffith, precise temperature regulation was in a different state from what it had been ten years ago. But the evolution of heat which occurred when a substance changed from the liquid to the solid state would affect the temperature in the immediate neighbourhood of the crystal, and, where rapid crystallisations were contemplated, it seemed very difficult to keep the temperature uniform in the

neighbourhood of the crystal considered. Even though it might be possible to keep the temperature uniform within very narrow limits in the mass as a whole, the local variations would be troublesome.

Prof. Thompson pointed out that though the paper treated only of singly refracting crystals he hoped that doubly refracting crystals might also be manufactured. A cubic foot of topaz, not to mention Iceland spar, would be extremely valuable.

He thought that the inventor of a method of manufacturing diamonds would be an enormous benefactor. The very valuable optical and physical properties of diamonds could then be used, instead of being wasted in pure luxury.

Mr CHALMERS said that this paper was of special importance in that it indicated a possibility of the solution of the great problem of glasses suitable for "apochromatic" objectives. In the course of some theoretical investigations he had found reason for believing that the problem of the production of a pair of glasses with a sufficiently great difference in the values of v and proportional dispersions could only be solved by a very great departure, either in form or composition, from the present types of glasses.

Mr Rosenhain's proposals would, he hoped, solve this problem.

Mr W. ROSENHAIN, in reply, said that he was very grateful for the way in which his somewhat sketchy paper had been received. He very strongly wished that some practical steps could be taken in the direction indicated in the paper. One must not expect to obtain large results, such as the "cubic foot of clear topaz" mentioned by Prof. Thompson, in a short time, but ultimately he hoped that crystals measured in inches might be produced, and even that would be a great achievement. The difficulty raised by Prof. Thompson, as to the heat evolved in the act of crystallisation was not perhaps so serious as it appeared at first sight, since this heat might be utilised to balance a part of the radiation loss. There were, however, very serious experimental difficulties to be overcome, so that rather elaborate appliances would be required, and it was doubtful whether the sums obtainable in grants would be adequate for the purpose. On the other hand, owing to the prolonged and costly nature of the research involved, and the difficulty of securing an adequate return under patents, it would be difficult to induce even the most enterprising firm to take the matter up as a commercial speculation. The only hope in that direction lay in the attainment of some preliminary results which should demonstrate the fact that theoretical expectations were being realised experimentally, and he (Mr Rosenhain) hoped ultimately to be able to do something in that direction. With regard to the theoretical difficulty indicated by Mr Chalmers as standing in the way of the production of a pair of "apochromatic" glasses having a considerable difference of v, he (Mr Rosenhain) would like to say that he had actually produced such a pair of glasses with a v-difference of nearly 20, in the laboratory, but here as elsewhere had met great difficulties in translating laboratory results into manufacturing practice.

MEASUREMENT OF REFRACTIVE INDEX.

By S. D. CHALMERS, M.A., Head of Department of Technical Optics, Northampton Institute.

THE refractive index of a piece of glass can be measured with very considerable accuracy by the aid of spectrometers of various types. It is possible to obtain a determination of the value of n_D with an error of not more than 1 in the fourth figure of the decimal, while the values of the dispersions from C to F, D to F, and F to G[1] will have an error of not more than 2 or 3 in the fifth decimal place.

These measurements require the glass to be converted into a prism of suitable angle, and are thus only applicable to a sample of the plate or melting from which the glass is taken. In many cases, however, it is of the utmost importance to be able to determine, with something like the same accuracy, these values when the glass is in the form of a lens.

Two methods which have been adopted are not susceptible of sufficient accuracy: the microscope

method is not reliable beyond the second figure of the decimal, even under the most favourable conditions; while the deduction of the refractive index from a measurement of the power, curvatures, and thickness of the lens demands the same proportionate accuracy in the power and curvatures as is required in the value of $n_D - 1$. This is evident from an examination of the formula employed—

$$F = F_1 + F_2 - \frac{t}{n}F_1F_2 \text{ where } F_1 = (n-1)R_1 \text{ and } F_2 = (1-n)R_2$$

F being the power of the lens, R_1 and R_2 the curvatures considered positive when convex to the incident light.

In practice the curvature cannot be measured with any certainty to more than 1 part in 1000, and with very small lenses the accuracy is very much less. Again, the effects of spherical aberration make it exceedingly difficult to obtain an accurate measurement of the back focus, while, with short-focus lenses, an accuracy of 1 in 1000 in the actual measurement of the distance requires exceedingly accurate apparatus.

To avoid the difficulties of this method the author places the lens in a transparent liquid of approximately the same refractive index, this liquid being contained in a vessel whose sides are made of plane parallel glass. The result is that we obtain, effectively, a lens of very much less power, in fact, a power which is the difference between the power of the original lens and that of an exactly similar lens with the same refractive index as the liquid.

Let n_1 denote the refractive index of the lens for any chosen wave-length, n_0 the corresponding refractive index for the liquid. Then, when the thickness is small compared with the radii

$$F = (n_1 - n_0) \cdot (R_1 - R_2),$$

and it is only necessary to determine the values of F and R_1, R_2 to the degree of accuracy that is required for the value of $n_1 - n_0$.

Thus if the value of $n_1 - n_2$ is about ·02, an error of 1 part in 200 in either of these determinations will correspond to 1 in the fourth decimal place in the final value of n_1. An additional advantage of the method is that difficulties arising from the spherical aberration of the lens are eliminated or very much reduced, while it is, in general, possible to neglect the thickness in calculating the value of $n_1 - n_0$, and even in the cases where this is no longer possible the effect is very small and readily obtainable.

To use the method to the best advantage, a series of stable transparent liquids of suitable refractive indices is required. It would save trouble if the refractive indices of the liquids could be relied on when used on different occasions and at different temperatures; but it is possible to verify each liquid by the aid of a lens of known glass.

The author has used, for most of his experiments, Cedar Oil ($n_D = 1\cdot517$ approximately) and Oil of Cloves ($n_D = 1\cdot530$ approximately); but experiments are being made to discover suitable liquids of higher refractive indices in order to obtain more accurate results with Dense Flints.

The curvatures of the lenses can be measured with sufficient accuracy by the Abbé spherometer, or in the case of lenses of small diameters, by the method described by Dr Drysdale;[1] the value of the thickness need only be approximately known.

The measurement of v, which is practically $= \frac{1}{F}$, may be made in a number of ways, the particular method employed depending on the size and focal length of the lens to be measured. Three distinct methods have been used for this purpose. In the first an Optical Bench[2] is used; either a distant source of light or, by preference, a collimator being used. The liquid trough is placed so that the lens is co-axial with a subsidiary lens screwed into the lens-holder, and between it and the collimator; it would be desirable to place the trough practically in contact with this subsidiary lens, but if the separation be measured, its effect can be readily allowed for. The image of the source of light formed by the subsidiary lens is observed and focussed by the microscope. The lens is then inserted in the liquid, and the change in the position of the microscope necessary to refocus this image is measured with the greatest possible accuracy. If the subsidiary lens is of suitable focus, and the most suitable liquid is used, this quantity should be small and the accuracy very great. If this distance be denoted by d, and a and b be the distances from the subsidiary lens to the trough, and the first

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[2] The Beck Lens Bench was used for this purpose.

focus respectively. $\dfrac{d}{a(a+d)}$ represents the difference in curvature at the subsidiary lens produced by the test lens. The value of v is then $\dfrac{a(a+d)}{d}+b$. As d will in general be very much smaller than a or b, it will not be necessary to measure a or b with any great accuracy.

The annexed Table shows the results of a series of measurements made with the aid of two liquids; in general an accuracy of ·0005 is obtained, while in some cases the errors are much less. They are within the range of possible variations of the melting from which the glass was taken.

When, however, extreme accuracy is desired, the exact position of the focus is determined for the same zone of the subsidiary lens (with and without the test lens) by the Hartmann method of extra focal measurement.

The preliminary measurements of the author indicate that the values of the dispersion can be obtained with an accuracy approaching that of spectrometer measurements.

For the purpose of simplifying the apparatus and increasing the accuracy of the method, an auto-collimating eye-piece is used, the subsidiary lens being both collimator and telescope objective. It is found desirable to incline the two faces of the trough at an angle of about 30 degrees, and to arrange an adjustable mirror so that the light after passing through the trough can be reflected back along its own path; this converts the whole apparatus into an auto-collimating spectrometer, which gives a spectrum of the source of light, each line being at minimum deviation when it is in the centre of the field. This device enables us to detect any variation in the refractive index of the liquid during the observation.

By measuring the amount of rotation of the mirror necessary to bring the various lines on the cross-wires, the dispersion of the liquid can be directly measured, or it can be checked on an Abbé spectrometer.

The equivalent focal length may be determined by the Fokometer method, *i.e.* the measurement of two magnifications of objects at a fixed distance apart. This method is suitable for small lenses where the refractive index for sodium light is wanted, and great accuracy is not required.

When suitable liquids are available, and these are carefully standardised and kept at a definite temperature, it should be possible to obtain a refractive index correct to ·0001, and the dispersion to the same order of accuracy as is obtainable with the Abbé spectrometer or Pulfrich refractometer.

MEASUREMENT OF REFRACTIVE INDEX.

No.	Glass.	Melting Value of n_D.	Curvatures of Lens (measured).		Bench Readings.		Curvature Difference.	Curvature produced by Lens.	$n_1 - n_0$.	n_1.	Difference.	Remarks.
					Without Lens.	With Lens.						
	LIQUID CEDAR OIL ($n_D = 1\cdot517$ approx.) standardised by Lens No. $1 = 1\cdot5173$.				cms.	cms.						
1	H.C.	1·5168	+13·42	−13·42	50−14+4·60	50−14+4·81	−·013	−·013	−·0005	1·5168	assumed	The separation of the test and subsidiary lenses is 1·5 cm. throughout.
2	H.C.	,,	,,	,,	,,	,, ,, +4·77	−·010	−·010	−·0004	1·5167	·0001	
3	H.C.	1·5175	10·16	−10·16	,,	,, ,, +4·53	+·004	+·004	+·0002	1·5175	·0000	
4	H.C.	1·5192	27·25	−27·25	,,	,, ,, +2·53	+·132	+·132	+·0024	1·5197	·0005	
5	D.F.	1·6118	−13·26	+·15	,,	80−·25+1·95	−1·229	−1·251	+·0933	1·6106	·0012	The object being at 5 m. a correction has been applied for change of object distance.
6	D.F.	,,	−13·26	+·00	,,	80−·25+1·15	−1·217	−1·239	+·0935	1·6108	·0010	
7	D.F.	,,	−9·49	+·17	,,	70−13·91+8·10	−·899	−·911	+·0943	1·6116	·0002	
8	L.F.	1·5481	77·91	+21·75	,,	36−13+5·81	+1·731	+1·688	+·0306	1·5479	·0002	
9	H.C.	?	16·87	+11·85	,,	50−14+3·94	+·041	+·041	+·0015	1·5188	·0004	Probably $n_D = 1\cdot5192$
LIQUID CLOVE OIL ($n_D = 1\cdot530$ approx.) standardised by Lens No. $1 = 1\cdot5316$.											
1	H.C.	...	...	...	50−13·98+4·35	50−9+7·38	−·393	−·395	−·0148	1·5168	assumed	
6	D.F.	...	...	...	...	60−0·25+10·50	−1·046	−1·061	+·0800	1·6116	·0002	
8	L.F.	...	...	...	...	80−13·8+2·50	+1·025	+1·010	+·0180	1·5486	·0005	

Mr Rosenhain considered that the method would be of great value in certain special cases; its general utility would depend on the accuracy with which the values of ν and the partial dispersions could be obtained. If this accuracy were sufficient, the advantage of using a lens instead of a specially worked prism would be sufficiently great to ensure the use of the method in many cases.

He foresaw serious difficulties arising from the large variation of the optical properties of liquids, especially the denser liquids, with the temperature. Convection currents would also interfere with the accuracy of the results, but their effects might be minimised by using the smallest possible thickness of liquid.

Mr Beck said this method would be extremely useful in determining the refractive index of worked glasses. The problem was frequently of great importance to the practical optician, because accidents occurred at various times, and lenses were made up out of wrong glass. Sometimes the lenses were only made up out of glass very slightly different in refractive index, and the amount of trouble in discovering the original error was very great. One had perhaps 300 lenses together, and a certain number had been made out of glass slightly different, and were just sufficiently wrong to be noticeable when completely put together, and not before. In a case of that sort such a test would be of very great value. The method of using a medium with a refractive index somewhat similar to the glass was not absolutely new. He really intended at one time to make up a set of standard media; the suggestion was to take cinnamic ether and reduce its refractive index by adulteration; monobromo naphthalin was also suggested. The idea was to keep a standard series of these troughs, and simply dip the lens in and get a neutralising effect. The accuracy was not anything compared with that of Mr Chalmers, but it was sometimes convenient. It was found, however, that the risk of the refractive index changing was too great, and that it was really not worth going on with. It would be interesting to hear whether Mr Chalmers was troubled by the surface of the trough at all in the work, or whether he allowed for that by taking the lens out and trying the trough without.

Prof. S. P. Thompson suggested the use of cinnamic ether as one of the denser liquids.

The Chairman : The idea of measuring the refractive index to this high degree of accuracy after the glass has been worked into lens form, was a very important departure, especially if the value of ν could be measured.

Mr Chalmers, in reply, said that he had taken special precautions to correct the temperature errors—the liquid holder was arranged in the form of a prism, which was placed practically in the position of minimum deviation; by this means the movement of the image in the field of the telescope could be made to indicate the change in the deviation of the liquid prism, and the temperature correction could be made. The convection currents did not with the troughs used seriously affect the definition, but it was important to keep the *outside* of the troughs scrupulously clean. The accuracy of measurements of dispersion could only be determined with the specially designed apparatus which was being made.

NOTES ON OPHTHALMOMETRY, WITH A DESCRIPTION OF AN IMPROVED INSTRUMENT.

By W. ETTLES, M.D., F.R.C.S., Ed.

A GLANCE at the general appearance of the ophthalmometer which I have the honour of submitting to your examination to-day will show that it differs materially from the Continental and American instruments. For these departures, Mr Charles Lees Curties and I are jointly responsible. So far as its optical structure is concerned, there is less dissimilarity, since the principles of ophthalmometry must necessarily obtain throughout the various models. This is not equivalent to saying that the glass-work is the same in all, for we have found the widest differences in material and correction for optical defects.

In the present instrument every modification that experience has shown to be a real improvement has been incorporated, together with some we have found it necessary to introduce so that it might fulfil those two great desiderata of scientific work : speed of execution and accuracy of registration. Apart from these considerations, we feel it can claim some measure of your attention as the first ophthalmometer made entirely in this country, and placed on the market as a serious competitor of the instruments from France, Holland, Switzerland, Germany, and the United States.

Beside the submission of this apparatus I desire to outline in the simplest possible way the optical principles it embodies. I have been led to attempt this, for all the descriptions I have seen are either so entangled in mathematical riddles as to leave us very much where we were, or aim at a severe simplicity by the easy but not very luminous method of leaving the difficulties unexplained.

The object of the ophthalmometer is the measurement of the radius of curvature of the cornea in any given meridian. When we know this amount, and the index of refraction of the dioptric medium, bounded by the curvature, we can estimate the refraction in the plane of that meridian. By comparing this with similar measurements made in other meridians, we can say whether the cornea is astigmatic or not, and if it is we can find the degree and axis of the error.

The basis on which the radius of curvature is calculated is the alteration in size of an object until its reflected corneal image attains a known and constant magnitude. There are four factors in our equation, and the measurement of any three will enable us to find the fourth. The three we measure are (1) the size of the object, (2) the distance of the object, (3) the size of the image.

The cornea is by no means a truly spherical convex mirror. It cannot, indeed, be accurately represented by any known geometrical figure. The nearest is an ellipsoid of rotation about its major axis. In the astigmatic cornea we have at least three unequal axes. Broadly speaking, it is divisible into a small and more or less spherical cap situated at the vertex and transfixed by the visual axis, and a more flattened or pyramidal basilar zone which constitutes its periphery.

The part most concerned in vision, and therefore in clinical ophthalmometry, is this polar cap whose radius is about 1·5 mm. When the observed eye has its visual axis coincident with the optic axis of the instrument, the aperture of the convex mirror the cornea constitutes is identical with the polar cap.
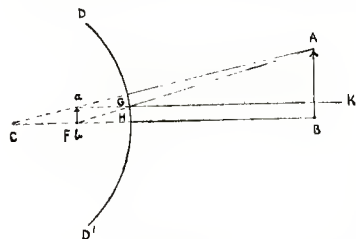
For the present we will assume it to be spherical, in which case the image will be formed in the following manner : Let us take a spherical surface DD[1] (Fig. 1) of which C is the centre of curvature and F the principal focus, midway between the centre and the vertex. In front of this surface we have an object AB, from which rays proceed in all directions, and some of which will encounter our mirror. The image will be formed at the intersection

Fig. 1.

of any two rays from a given point.

Thus we have—(1) A ray AC directed to the centre of curvature will be reflected on itself.

(2) A ray AF directed to the principal focus will be reflected parallel to the optic axis CB, and its prolongation will intersect AC in a. From this point we drop a perpendicular ab, and so obtain our image of the object AB.

Since aK is parallel to bB, the distance ab is equal to the distance GH.

Now consider the triangle AFB, in this

$$\frac{AB}{GH} = \frac{FB}{FH}.$$

As the utilised aperture of the mirror is very small, we may assume that $FG = FH$, i.e. $FH = \frac{CH}{2}$ or $\frac{R}{2}$.

If we term the object AB, O ; $ab = GH$, I, and BH d, we get

$$\frac{O}{I} = \frac{d + \frac{R}{2}}{\frac{R}{2}}$$

or, as d is very great compared to $\frac{R}{2}$, we may express it as

$$\frac{O}{I} = \frac{2d}{R} \text{ or } R = \frac{2d\,I}{O}.$$

Let us apply this generalisation to a practical experiment. We take a small spherical mirror, such as this 1-inch steel motor ball. In front of it we place a short focus astronomical telescope (450 mm.) so that we can view the virtual image. In the arc of the circle whose radius is the focal length we place an object.

Inasmuch as we only measure and regard the ends of an object we need not use a solid bar, but two luminous slits which will represent its terminals. We place these equidistant from the optic axis, and in contact with the mirror we place a short scale graduated in half millimetres.

Let us take one of many trials. The ball is 1 inch in diameter or 25·4 mm., and its radius therefore is 12·7 mm., as measured by callipers.

The distance between the slits, i.e. the object, is 417 mm. The distance of each and the front of the telescope from the mirror is 450 mm. The linear magnitude of the image as shown on the scale is 5·8 mm.

By using the formula, $R = \dfrac{2\left(d + \dfrac{R}{2}\right) \times I}{O}$, we get 12·59 mm., and the mean of a series gives us

12·65, or a difference of five hundredths of a millimetre.

With the simpler formula, $R = \dfrac{2d\,I}{O}$, we get a mean of 12·6, so that it gives an approximation quite near enough for practical purposes.

As soon as we replace our steel ball by a living eye we encounter difficulties. Firstly, we cannot place a scale in contact with it. That is overcome by placing it in the focal plane of the eye-piece.

The second is that the eye cannot remain sufficiently steady to permit accurate readings. This can only be partially eliminated by intelligent assistance on the part of the patient, and by taking the mean of a number of observations.

This method which you will observe embodies all the essentials of ophthalmometry, and the one drawback to which is the unsteadiness of the image, was that adopted by Kohlrausch in 1839. By means of it he and Senff determined with great accuracy the radius of curvature of the human cornea. It is important to bear this in mind, because all the works dealing with the ophthalmometer, or nearly all, ascribe the credit of its invention to Helmholtz. That is clearly wrong, for Helmholtz did not publish the description of his ophthalmometer until 1854. I do not desire to underrate his splendid services to physiological optics, but in weaving a garland to his memory we must not pluck the hardly won laurels of other workers.

We have seen that the drawback of the Kohlrausch method is the unsteadiness of the image, a defect which was entirely overcome by the ingenuity of Helmholtz, who utilised a method long known to astronomers, and, incidentally, to our immortal Thomas Young, by means of which the diameter of a moving star could be readily ascertained.   The principle utilised is that of double images.

Suppose that a planet is under consideration.   By doubling it we obtain an image like this (Fig. 2).   The moment we obtain exact contact we know that the periphery of the central image passes
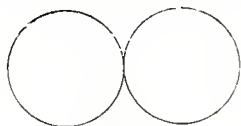
Fig. 2.
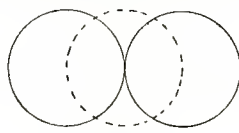
Fig. 3.

through the centres of curvature of the double images (Fig. 3).   This doubling was carried out by placing in front of the objective of the telescope two parallel-sided glass plates which could be rotated in opposite directions symmetrically to the axis of the instrument.

When the plates are in the primary position, rays from an object O (Fig. 4) pass through them without deflection.   If, however, a plate be inclined, the incident ray will be refracted towards the
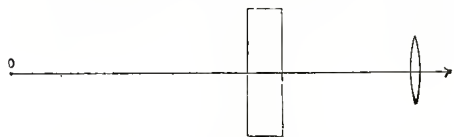
Fig. 4.
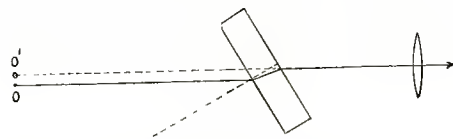
Fig. 5.

normal, and on emergence will be restored to its original direction, but deflected from its original path, and this deviation will be proportionate to the thickness of glass traversed.   The image will be projected in the path of this deflected ray, so that the object will appear to be situated at O′ (Fig. 5).   Similarly, if the other plate be symmetrically displaced, O will appear at O″ (Fig. 6) so that O will disappear, and O′ and O″ take its place.   By rotating the plates so that the margins touch the size of O becomes known.
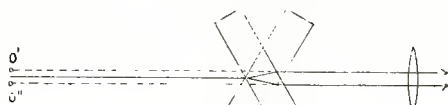

Fig. 6.

There are other methods in vogue.   Thus Kagenaar of Utrecht splits the cone of light by using a bi-prism with the apices in contact.   There is little loss of light, but there is only one position in which the best results can be obtained.   Another method is to use a doubly refracting plate of Calcite (Iceland spar), but in this one image is centric and another is eccentric.   The best method, although an expensive one, is to use a Wollaston prism.

It has the advantage that each individual ray is split and deviated symmetrically to the incident one.   It consists of two right-angled prisms of quartz or calcite, placed base to apex, and with th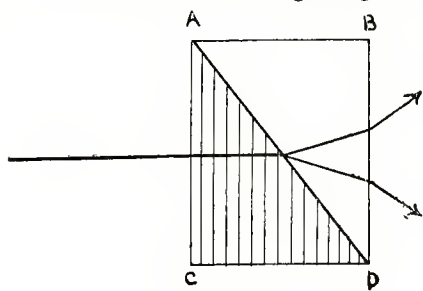e hypotenuses cemented together.   In the one prism, however the apex is parallel to the axis of the crystal, while in the other, the apex is at right angles to the axis.

Thus in the figure ABCD (Fig. 7), the optic axis of the prism ACD is in the plane of the diagram, while in ABD the axis is at right angles to it.

A ray of light incident on the face AC and normal to it becomes divided into two rays, one ordinary and one extra-ordinary.   These will travel with different velocities.   The vibrations of the ordinary ray are in a direction perpendicular to the axis of the crystal (and to the diagram).

Fig. 7.

Impinging on the plane AD its direction will change, and this ordinary ray now becomes extraordinary in the prism BDA, for, in this, vibrations perpendicular to the plane of the diagram will be parallel to the principal plane. The effective index of ABD may be obtained in this way. Let μ_1 stand for the refractive index for the ordinary ray, and μ_2 for the extraordinary, then

$$\frac{\sin i}{\sin r} = \frac{\mu_2}{\mu_1}, \ i.e. \ \frac{1 \cdot 558}{1 \cdot 548}.$$

If we follow the extraordinary ray in ACD, we find it oscillating in the axis of the prism (and in the plane of the diagram), but on passing AD it becomes ordinary.

From this ray we have—

$$\frac{\sin i}{\sin r} = \frac{\mu_1}{\mu_2}, \ i.e. \ \frac{1 \cdot 548}{1 \cdot 558}.$$

Thus we find that the extraordinary ray is refracted towards the base of the prism, and the ordinary toward the apex.

On emerging from the bi-prism, the rays are still further deflected from the normal.

In the ophthalmometer the prism is situated 290 mm. from the cornea and the eye-piece, and as the image is a constant one of 2·94 mm., we find its deviation is—

$$\frac{2 \cdot 94}{2(290)} = \tan d.$$

We will assume then that by means of some such method, preferably a Wollaston prism, we can produce a constant bilateral deviation, how is it that this property may be utilised in measurements? Let P be such a prism through which rays pass from an object O (Fig. 8).

Where the rays intersect at a, O′ and O″ will be in exact contact, and as the rays are parallel, O, O′, and O″ are all equal.

Whenever O′ and O″ just meet at a distance d from the prism, they indicate that O is a constant known size, which can be calculated from the angle of the prism and the distance d. You will observe that there are two invariables, the angle and the distance.

If we increase the size of O we increase d and *vice versâ*. This gives us two methods of measurement.

Fig. 8.

(*a*) We can have an object of fixed dimensions and move the prism so as to measure the distance from it at which contact is obtained as is done in the Chambers-Inskeep instrument.[1]

(*b*) We can have this distance at which contact is to take place a fixed quantity, and modify our object until that contact occurs.

That is Javal's method, and is the one we have adopted. It should be borne in mind that in looking through the instrument we do not see the image from which our calculation is made. The object is the interspace between the inner edges of the luminous wires, and the image is therefore the interspace between the reflections; when contact takes place there is no interspace, and, consequently, we are regarding not the image, but the index of the image.

I have here a model or skeleton ophthalmometer. It consists, as you see, of a small steel convex mirror, 7·5 mm. radius, placed at the anterior focus of the front objective. Between the two objectives there is a sliding carrier bearing the bi-prism. The eye-piece is focussed on the image formed by the back objective. Rays from our artificial cornea will be parallel in the space between the objectives where they encounter the bi-prism, and these parallel rays are brought to a focus to produce an inverted image identical in size with the corneal one. The optical arrangement is not therefore either a microscope or a telescope, strictly speaking. It is simply an optical arrangement by which we take the corneal image from one position to another, so that it may be more conveniently magnified and examined by the tiny microscope we can regard the Ramsden eye-piece as being.
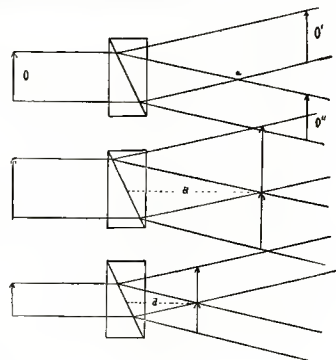
[1] The same method had previously been used by Landolt in his *diplomètre* (Javal, " Mémoires D'Ophthalmométrie,"

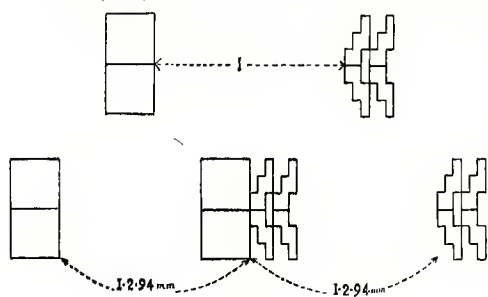Well, if you slide the prism aside you will perceive the two reflections of the mires (Fig. 9). The space between them is the image, and this may or may not be the size required. To see if it is, slide the prism into its place, and you will now perceive four images. If the two central ones coincide you know for a fact that the interspace is 2·94 mm. in length, for only such an image will exactly double with a half degree bi-prism at the focal length of the second objective. If they do not coincide, you move the candles until they do, and the distance between the flames is the measure of the object.

Fig. 9.

We now know (1) that when the central portions of the doubled image are in contact, that image is 2·94 mm. in length; (2) that we can secure this contact by altering the size of the object, and that the amount of such alteration is the reciprocal of the radius of curvature; but we do not yet know how that figure 2·94 has been arrived at.

It is one of the many important advances made by Javal, late Director of the Ophthalmological Laboratory at the Sorbonne, to whom we owe so much in this department of science, and with whom we all most deeply sympathise in the affliction which has overtaken him, and which, indeed, has overtaken us as well in that it deprives us of so great a leader.

Javal's idea was to exclude all computations, and make the indices of the instrument record at once the dioptric value and the radius of curvature of the meridian under observation. That has been worked out in the following manner, and I may say at once it is the basis on which our instrument has been graduated.

The cornea is a curved surface bounding a more or less homogeneous refracting medium.

The refractive value of such a surface is the inverse of its anterior focal distance.

$$D = \frac{1}{F_1}$$

In its turn the anterior focal distance can be expressed by the formula—

$$F_1 = \frac{R}{(\mu - 1)1000}.$$

By substituting this for F_1 and inverting, we get—

$$D = \frac{(\mu - 1)1000}{R}.$$

Now, what is the refractive index? It has been variously estimated, but the differences are not great, and Javal assessed it at 1·3375.

Substituting this value for μ, we find that—

$$\frac{337·5}{R} = D, \text{ and } \frac{337·5}{D} = R.$$

The length of the radius has been found to vary from 6·5 to 9 mm., that is to say, the dioptric value ranges from 37 to 52 D. It is thus a simple matter to calculate the radial value of each increment of a dioptre between these limits, and, conversely, to express the dioptric value of each fraction of a millimetre.

We have seen that the alteration in size of the object—that is, the excursion of the mires—is the reciprocal of the curvature, and Javal's next step was to make each degree of angular movement equivalent to one dioptre.

If we hark back to our fundamental equation, $\frac{O}{I} = \frac{2d}{R}$, we have $O = \frac{2d\,I}{R}$. But we have found a new value for R, viz. $\frac{337·5}{D}$, and substituting this we get $O = \frac{2d\,ID}{337·5}$.

I is still an unknown quantity, but it stands for the amount of the doubling of the bi-prism, so that the margins of the image are in contact at the second focal distance. Let us call this doubling β, and substitute it for I.

$$O \cdot \frac{2d\,D\beta}{337\cdot5}.$$

We have further to express the size of the object in terms of the linear value of one dioptre measured on the arc. Call this unknown quantity x. Then our object will be x dioptres.

$$Dx = \frac{2d\,D\beta}{337\cdot5},$$

and by eliminating D

$$x = \frac{2d\,\beta}{337\cdot5} \qquad\qquad . \qquad . \qquad . \qquad (1)$$

This linear value of 1 D is to be equivalent to the linear value of $1°$ on the arc.

$$\frac{1°}{360°} = \frac{x}{2d\,\pi}, \text{ or } x = \frac{2d\,\pi}{360} \qquad . \qquad . \qquad (2)$$

Combining these, we have—

$$\frac{2d\,\beta}{337\cdot5} = \frac{2d\,\pi}{360}, \text{ or } \beta = \frac{337\cdot5\,\pi}{360}, \text{ or } 2\cdot94 \text{ mm.}$$

To sum up, then, with two objectives, each of 270 mm. focal length, separated by an interspace of 40 mm., containing a bi-prism of 30 minutes doubling an image 2·94 mm. at the second focal point, each degree of arc will be equivalent to a refractive power of one dioptre of corneal curvature.

Description of the Instrument.—Having briefly sketched the principles of the optical construction and graduation, it remains for me to describe in some detail the mechanism of the instrument before you.

It is supported on a tripod resting on the floor. The hollow upright is fitted with a solid plunger, which rests on a spiral spring. This spring rather more than counterbalances the superincumbent weight, so that the apparatus rests in a position of considerable elevation. By means of very slight pressure applied to the central block terminating the upper end of the plunger it can be adjusted to any desired height instantaneously, and there rigidly fixed by the clamping screw.

Any one conversant with the ordinary instrument knows how difficult it is to obtain this most essential adjustment to the patient's height, and to obviate its various rising and falling, tables have been introduced. The original model was a table instrument, and throughout all its various shapes that feature has been retained. Why, it is hard to say, but a similar conservatism is shown in the history of most inventions.

We have definitely discarded the table, and the floor instrument not only possesses all the advantages of the combined instrument and falling table, but has superiorities in cheapness, in rigidity, and in lightness which they cannot claim.

It is a matter of an instant only to adjust the apparatus to the exact position.

The head-supporter is a framework of cast steel with a movable chin-rest. This framework is carried on a triangular steel bar which slides horizontally in the metal block. Working in a groove in this bar is a shaft, turned by a milled head at the operator's end, and by means of a bevel gear having its rotation transferred to a shorter vertical shaft. This, in its turn, is furnished with a screw thread engaging in a sliding block bearing the chin-rest.

Hitherto the chin-rests have been raised by means of a bell-crank lever, the long arm of which strikes up the chin block. There is so much mechanical loss of power in this that one is unable to raise it with the head in position without bending the lever or straining its bearings. In our apparatus the power of the screw is such that the pressure of the head causes hardly any sensible resistance.

The chin-rest is deeply hollowed. It has been found that it is not enough to support the head vertically, it must also be prevented from sliding forward. The chin-rest is made of non-conducting material, and the band against which the forehead is applied is also lined, so that if there is any earthing of the current, as takes place in badly insulated installations, especially on wet days, the patient is saved

that unpleasant pricking one often meets with in nickel-plated rests. The occluding disc swings over to cover one or other eye as required, and it bears a standard steel ball of 7·5 mm. radius, on which the telescope may be focussed and its accuracy of reading checked from time to time. The vertical sides of the face-piece bear broad white lines where the outer canthus of the eye should come. The usefulness of this lies in the fact that by noting the level of the patient's canthus the plunger can be driven down to that height and secured so that the eye is at once in position, and but very little subsequent adjustment is required. On the side opposite the patient the centre block carries a very strong triangular steel bar. This is fitted with a rack, and on it another block slides by means of a pinion and large double milled heads. By means of this the optical arrangement can be focussed without any vibration.

This sliding block carries a female pillar, with a second rack and pinion. Engaging in it is a male rack so that vertical adjustments can be made with equal ease and accuracy. The upper end of the vertical rod is hollowed for the reception of a short spindle, thus affording circular movement in the horizontal plane. By means of a stop screw, it can be fixed where desired. We thus secure antero-posterior, vertical, and horizontally circular adjustments, all of which when made leave the instrument absolutely rigid.

Ettles-Curties Opthalmometer.

The necessity for this is apparent to any one who has found the instrument deviate just as he was in the act of swinging the arc into the secondary meridian. Another advantage is that the eye remains in the primary position throughout. The Continental instruments secure their vertical adjustment by dipping the telescope in the vertical plane. This not only causes the eye under observation to assume a secondary position, but it interposes the lashes of the upper lid, so that the image of the upper mire when the arc is vertical is dimly seen and is often invisible.

"Sighting" the eye is seldom necessary, but if it is desired two sights are provided, one through a peep-sight in the axis needle, the other just over the distal end of the tube.

The telescope tube rotates within a strong sleeve, and is furnished with a Ramsden eye-piece and cross thread. This is an essential feature. There are many instruments—notably American ones—with Huygenian or fixed oculars; but since each error of 1D on the part of the operator means an error of 0·33D in the final estimation, it will be seen that they introduce an error which is the less excusable because it is so easily eliminated. The eye-piece rim is also insulated to protect the operator from earthing a current. The fixed sleeve carries a large transparent goniometer, while the rotating tube is furnished with a pointer. The axis of the meridian can thus be read at a glance.

You will note that the long hollow cone at the end of the tube, and the large black shield, are conspicuous by their absence. Their presence in these days of trans-illuminated mires is another evidence of that curious conservatism of design I spoke of. Formerly, when the mires reflected day-light or artificial light, the light source was in such a position as to stream more or less directly into the tube, while the surrounding structures were also illuminated and their images caused confusion. Now that the only light used is that enclosed behind the mires they are useless.

The arc is a stout metal quadrant bearing the doubly movable mires which are actuated by oppositely moving racks engaging in a pinion and cylindrical driving-nut.
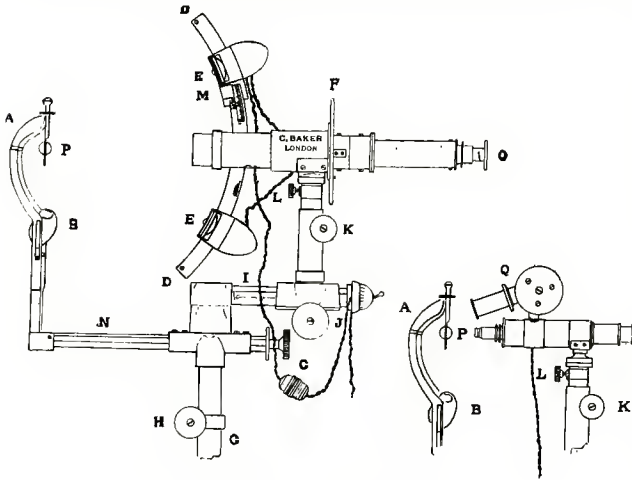
The right-hand side of the arc bears the gradations in dioptres and millimetres of radius. The mires are cut out of metal as stencil plates, which is far superior to blackening glass. The heat causes it to peel off in a short time.

Each mire is 60 by 30 mm., one being a parallelogram, the other being stepped in steps of 10 mm. by 5 mm. The black line transfixing each can be set exactly by a set screw, though this is perhaps an unnecessary refinement. Each stencil is evenly illuminated by a pane of opal glass.

For some time we used a canary-coloured glass, being more or less monochromatic in the illumination of our wires. The great advantage of this is that one overcomes the chromatic dispersion of the bi-prism, and so can obtain a much greater accuracy in deciding on contact. In my opinion the advantage of monochromatic illumination is hardly secondary to that of trans-illumination. By means of the combination of these, accuracy is obtained to nearly the third decimal point. As the classic instrument of Helmholtz cannot guarantee the second decimal, you will see it has to give way on the one point it has hitherto been regarded as possessing superior qualifications.

We need not be confined to any definite hue of monochromatic light, and in this instrument we have followed the suggestion of Pflüger, and used complementary colours. We retain the freedom from fringes, and we obtain a more or less pure white when the images are superposed. This facilitates the reading of contact and of overlapping.

The mires are provided with hoods open above and below, and containing powerful electric light bulbs of cylindrical shape.

By being so open the metal work gets much less heated than when completely enclosed, while the stray light gives sufficient illumination for the readings and manipulations.

The right-hand mire hood is perforated at the side, and bears a tiny mirror. This reflects the scale, which is always clearly in view of the operator, who need not rise from his seat to make the observations. The index is a fine line between an open gap like the cursor of a slide rule, so that no part of the scale is covered up, and fractions are much more easily read. This procedure of illuminating the scale, using a thread index and erecting it, is an entirely novel feature, and has proved of the greatest service.

The objectives have a large working aperture, and are identical in their construction, and are corrected for aberration. The bi-prism is a Wollaston most carefully ground and standardised on a refractometer.

At first we feared we should have to go abroad to have the prism worked, but it is a matter of gratification to me to be able to say that the whole of the apparatus before you has been made in this country, and there is no difficulty now in obtaining these in any quantity desired.

Although not an essential portion of the ophthalmometer, it behoves me to say something of the corneal microscope. When you come to think of it, you require for the use of this instrument rigidity of fixation of the patient's head, and some method by which the optical part may be moved up and down, forward and backward, and that delicately. Well, all these movements we already possess in the ophthalmometer. All we have to do is to loosen the screw holding the pin of the ophthalmometer and lift it away, and then substitute a microscope supported on a similar pin. At once we possess as perfect a mechanism as one could desire. The long triangular bar supporting the face piece is slid home, so that the objective is now within its focal distance.

Any objective or ocular may be used, for the body is of standard continental size. Illumination is provided for by a little search light which produces a diffused or concentrated beam in any direction at will. The metal eye is very useful here, as the light arrangement may be made on it before subjecting the living eye to its rays. The results obtained are quite startling to those seeing an eye for the first time under these conditions. It would be beyond the scope of this paper to deal with them, but I may be permitted to say that it opens up a new and most fruitful field in the study of eye disease.

You see we have a good deal more than an ordinary ophthalmometer here. It represents an ophthalmometer, a rising and falling table, and a corneal microscope. It is cheaper than the total cost

of these, and, what is of much greater importance, is capable of far better scientific work than these can accomplish collectively or individually.

Mr H. L. TAYLOR congratulated the author on the production of a British Ophthalmometer with such important mechanical improvements; the arrangement for the patient's head was, he thought, a great improvement, avoiding as it did physiological effects, such as "stare" astigmatism due to the constrained position of the head. He suggested that a small screen should be placed in front of the pinion which moved the rack, as the movement of the hand was always liable to distract the patient's attention.

The Ramsden eyepiece with cross wires was a decided improvement on the common forms. He thought the mires should be horse-shoe or circular in pattern, as this would enable the observer to see the main defects of the eye, so far as they were due to the want of sphericity of the cornea, with very great ease. The use of complementary colours was, he thought, a very happy idea, and was likely to be very useful in practice.

Mr CHALMERS suggested that the corneal microscope might be fitted with a vertical illuminator, thus permitting of the direct illumination of the eye under examination.

Dr ETTLES, in reply, said he had hoped for further criticisms, and still hoped that such criticisms and suggestions should be brought to the notice of Mr Curties and himself.

SATURDAY, JUNE 3.

SECTION II

Dr C. V. DRYSDALE in the Chair.

THE PRINCIPLES OF TRICHROMATIC PHOTOGRAPHY.

By A. J. Bull, of the L.C.C. School of Photo-engraving.

The most successful processes, up to the present, of photographically reproducing the colours of objects are those founded primarily upon the fact that all common colours can be imitated by mixtures of the three primary colours, red, green, and blue.

Processes of this class readily fall into two groups—those in which the final picture is produced by the addition of the primary colours by projection, as in Ives' Kromskop (positive synthesis), and those where the primary colours are absorbed by the pigments used for printing, such as Lumiere's transparency process and typographic block-printing (negative synthesis). This latter is the only aspect of the subject which has assumed much commercial importance.

Colour-Mixture Curves and the Reproduction of the Spectrum.—The idea of reproducing the spectrum by a photographic process founded on Maxwell's colour-mixture curves is a simple one. The ordinates of these curves give for every part of the spectrum the proportions of the three primary colours necessary to give the closest match to the hue. If three photographic negatives of a spectrum of white light could be obtained whose opacities corresponded exactly to these curves, and from these transparencies (positives) were made in which the transparency followed the same curves, and images of these were projected on a screen in their proper positions and illuminated each by its own particular primary light, then Maxwell's operation of matching each part of the spectrum by mixtures of red, green, and blue lights would have been accomplished photographically, and the reproduction would be perfect except for a slight degradation with the sensation of white in the yellow and blue-green regions and the loss of the violet if the pure blue had been taken as one of the primaries.

Before entering into detail regarding conditions imposed by the nature of ordinary colours and the properties of photographic plates, it may be well to review some of the numerous hypotheses that have been advanced upon this subject, of which naturally not more than one can be correct, bearing in mind that the usual problem is not necessarily to reproduce all mono-chromatic hues, but the colours which are commonly met with in Nature.

The principal hypotheses may be grouped under five headings :—

I. *Colour-Sensation Curves.*—These have been made the foundation of a few hypotheses. According to one authority, the densities of the negatives of a spectrum should follow these curves,[1] or the colours photographed should be those most representative of the primary sensations.[2] This latter would seem to indicate that they should be somewhat narrow bands.

[1] *Instruction in Photography*, by Sir Wm. Abney, 10th ed., pp. 348-350.
[2] *Sixth Traill Taylor Memorial Lecture*, by Sir Wm. Abney.

Dr Clay, taking Sir Wm. Abney's colour-sensation curves as a basis, has worked out the curves that should be followed by the negative records in reproducing the spectrum in order that the degradation of the colours should be with the sensation of white only. In this connection he points out that inks with abrupt absorptions give rise to purer sensations than those with gradual absorptions.

II. *Colour Mixture-Curves.*—Mr F. E. Ives, one of the pioneers of three-colour work, is the chief exponent of the idea that the density of the negative records should follow the colour-mixture curves,[1] Von Hübl admitting the possibility of the hypothesis being correct. Mr Ives' specification of the printing colours is that they should be those most anti-chromatic to the colours photographed, and evidently intends that the absorptions should be gradual.

Many writers have, unfortunately, in writing upon this subject, confused the sensation and mixture curves, although the functions represented are totally distinct.

III. *Negative Records derived from the Ink Absorptions.*—The method of adjusting filters and plates put forward by Von Hübl[2] consists in determining the "middle absorptions" of the printing colours and calculating curves for the negative records, whose maxima are in the same positions as these absorptions, and which terminate at the adjacent "middle absorptions." It is, however, interesting to note that he appears to make but little attempt to carry this idea out in practice, relying rather upon the indications given by a colour chart.

IV. *Equal Division of Spectrum.*—Both Dr Adolf Miethe and Mr Howard Farmer consider that the filters should divide the spectrum into three parts without any overlapping at about λ 6000 and λ 5000, although in one of his papers Dr Miethe describes a method of introducing an artificial overlap effect by giving short supplemental exposures through the other filters.

V. *Regulated Overlap.*—Two considerations, firstly that photographic action does not follow a straight line law, and secondly, that filter records following extended curves would reproduce certain colours with the addition of actual white or the adjacent spectrum colours, have led the author in conjunction with Messrs A. C. Jolley and A. J. Newton, to develop this hypothesis, and to follow it out in practice.

In this method of working negative records of a normal spectrum are made as even and to terminate as abruptly as possible. The region of the spectrum in which the red and green records overlap is reproduced by the printing colour of the blue negative, a yellow. Similarly the part photographed by both the green and blue negatives is rendered by the blue-green pigment used in printing from the red negative.

In any photographic process the ink, etc., is printed from the parts which are transparent in the negative, *i.e.* where the light has not been recorded, so that in printing in pigments these should absorb all the light photographed by the negative from which they are printed and be transparent to the light not so recorded. This gives the following inter-relation between the negatives and printing colours: That the sum of the colours of the region of overlap between the red and green negatives should accord as nearly as possible in hue with the sum of the colours not recorded by the blue negative, and also that the colours not recorded by the red negative should match those in the overlapping region of the blue and green negatives. These regions have been selected and matched visually,[3] but owing to the fact that in each case the colours having the more extended composition stimulate the third sensation more than do the colours of the overlapping regions, and also because these latter regions occur in those parts of the spectrum where the hue changes most rapidly, and is not always seen in exactly the same manner by different eyes, the matches cannot be made with any great degree of accuracy. It is, however, interesting to note that filters and plates adjusted on these principles have given under comparative test superior results to all other obtainable filters (twelve in number).[4]

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[1] *Third Traill Taylor Memorial Lecture*, by F. E. Ives.

[2] *Three Colour Photography*, by A. F. Von Hübl.

[3] "The Functions of Tri-colour Filters," by A. J. Bull, and A. C. Jolley (*Brit. Opt. Jour.*), Jan. 1904.

[4] "The Practical Performance of Tri-colour Filters," by A. J. Newton and A. J. Bull (*Photo. Jour.*). October, 1904,

So far as work along these lines has gone, it shows that the three negative records should, as far as is possible, extend evenly between λ 7000 and λ 5800, λ 6000 and λ 4600, and λ 5000 and λ 4000, the corresponding printing colours absorbing these regions and transmitting the remainders.

This system of three-colour reproduction tends in practice to render the varying hues of the spectrum by five patches of uniform colour.

Some Considerations of Negative Records intended to follow Curves.—The adjustment of a light-absorbing medium to correct the sensitiveness of any photographic plate so that the effect should follow some predetermined curve is a matter of great difficulty—so great that, in spite of several claims that have been put forward, it is a matter of great improbability that anything in this direction has ever been accomplished, the few filters of this type which the author has examined not being very successful.

Suppose, for instance, that a record was required with a maximum in the orange at λ 6100, the curve sloping in some particular manner down to the end of the red on one side and to some point in the green on the other. Now, as far as gelatine plates are concerned, the sensitiveness of even the best panchromatic ones is extremely low to the region λ 7000 to λ 6300, in but few of them can the record be pushed beyond λ 6500, unless they are screened from the action of the shorter wave-lengths and given greatly prolonged exposures ; so that it would be practically impossible to effect the adjustment required. Again the various bands of comparative insensitiveness that occur in the green and blue green with the best modern sensitisers would greatly increase the difficulty of making adjustments to curves in this region.

But if the adjustments were possible, the curve effect required could only be obtained when exposing to the spectrum at some one critical exposure and developing to a particular development factor, for since photographic action does not follow a straight-line law, any increase or decrease in the exposure will cause an alteration in the shape of the curve, the densities of the various parts not increasing or decreasing in the same ratios. Now an ordinary negative consists of a photographic plate which has received various exposures according to the light and shade of the original ; such a filter and plate would therefore be acting in a different manner in different parts of the same negative. Variations of this nature can be followed by giving various exposures to a spectrum, in proportion to the variation of light and shade in any picture. These variations have been found to be considerable in some cases.

It is for this reason that test-charts of light colours, such as those published by Von Hübl and by Eder, are of limited utility, since while they may be well rendered, a picture with greater contrasts of light and shade photographed on the same negatives may be so indifferently reproduced as to be almost useless.

Relation of Ordinary Colours and Monochromatic Colours as regards their Reproduction.—It does not of necessity follow that if a set of filters and plates could be made to reproduce a spectrum of pure colours exactly that it would render ordinary colours well, although this point is often assumed as being self-evident.

To render a spectrum correctly by positive synthesis the negatives must follow the colour-mixture curves. Negative synthesis would require either these same curves or some derived directly from them (probably the former), but the ordinary colours of objects would, because of their complex composition, be rendered too light in many cases. A red colour, for instance, which reflects light of the red end of the spectrum up to perhaps λ 6000, would be photographically recorded to some extent by the green negative as well as the red, instead of by the red only. So that in the reproduction insufficient crimson would be printed on the yellow, resulting in too orange a hue. An orange colour reflecting the red, yellow, and part of the green of the spectrum might be well rendered, but a yellow which reflects all the light between λ 7000 and λ 5000 would be rendered much lighter because of its being recorded in the blue negative. The case in which this effect would be the most marked would be that of a green colour.

Fundamental Requirements of the Reproduction Colours.—It is necessary, quite apart from their relations to the negatives, that the colours employed in building up the final pictures shall in themselves be capable

of reproducing all ordinary colours by suitable mixture or super-position. For positive synthesis they must be the primary colours— red, green and blue or blue-violet; but they need not consist of mono-chromatic lights only. Fairly broad bands of the spectrum may be taken provided that they are not more impure than any colours likely to occur in practice. Negative synthesis requires that the primary colours may be produced by superposing the printing colours in pairs—that is to say, the yellow and crimson should give a red as pure as any that are ordinarily met with, which means that these two colours between them must absorb the spectrum yellow, green, blue and violet, leaving the red and orange-red only. Similarly the superposition of the yellow and blue-green must effect the absorption of all parts of the spectrum not sensibly green in hue, *i.e.* the spectrum red, yellow, blue-green, blue and violet, and the crimson and blue-green must together absorb the red, yellow, green and blue-green. This indicates that the crimson to be used should absorb the yellow, green and blue-green ; the yellow should absorb the blue-green, blue and violet ; and the blue-green the red and yellow of the spectrum. This corresponds with the properties of the colours indicated by the method of regulated overlap.

Among the defects that occur in the reproduction colours for negative synthesis the most serious are in the blue-green, where there is generally a want of transparency to the green and an incomplete absorption of the red. These cause a darkening of the greens and a difficulty in obtaining blacks and greys free from a red tint.

The crimson is seldom as transparent to the blue and violet as it is to red, so that when superposed on the blue-green it produces a purple instead of a blue-violet, also as the spectrum yellow is often transmitted, the red given in conjunction with the yellow is too orange in hue. There is no difficulty in obtaining a suitable yellow.

In the staining of gelatine reliefs, fast green B.S., erythrosin, and brilliant yellow are very good dyes to use.

Inks for block-printing are generally much inferior in their properties to the dyes for making transparencies. In many cases the makers put forward inks which not only possess the defects mentioned in a most marked degree, but also the yellow is too pale, reflecting more blue than is necessary, and making the hue obtained by printing the blue ink on it a blue-green instead of green. Under these circumstances greens and yellow greens are only procured by additional fine etching. Another serious defect with trichromatic inks is that they are never sufficiently transparent, so that they have to be printed in the order of their opacities, which is usually yellow, crimson, blue.

The Mutual Adjustment of Filter Records and Reproduction Colours.—The adjustment of the filters to the inks, or the selection of suitable inks for certain filters, is often considered essential. The adjustment usually consists in making the filters reproduce the inks—that is, each filter and plate must record two of the inks equally while not photographing the third.

With the red negative there is no difficulty in recording the red and yellow inks equally, but in order to make the blue-green ink photograph equally with the yellow, the green record must be extended into the blue considerably, with a consequent loss of green. The effect of this is prejudicial to the reproduction of greens, as the whites are recorded with greater densities in the negative than any bright greens, so that more crimson is printed on the greens than on the whites.   ·

The blue negative, if it records the visible blue and violet only, will photograph the blue ink more than the crimson, because of the deficiency of blue in the latter. But as most of the crimson inks in use reflect more ultra-violet than violet, the records of these two inks may be approximately equalised by recording the violet and ultra-violet rather than visible light only. This adjustment produces its own defects on certain colours. Many reds, browns, and oranges, for example, reflect some ultra-violet, and this being recorded in the blue negative prevents sufficient yellow being used in their reproduction, thereby giving them a violet tint. Blue colours, too, may be affected. So that, if the filters and plates are adjusted to copy the inks when these are defective, incorrect effects will be introduced in the rendering of colours which differ from them. The better plan is to adjust the filters and plates to approximate as closely as possible to theoretical requirements, and to independently follow the same course with the reproduction colours.

Dr CLAY considered that Mr Bull was working on the right lines in endeavouring to reproduce colours of different degrees of brightness; that it was of the utmost importance to reproduce the same colour in the same way under different conditions of illumination, even if this necessitated the less perfect rendering of an evenly illuminated spectrum. Our aim was to reproduce natural colours, which consisted of large broad bands of mixed colours, which might be very unevenly illuminated. There was no doubt that plates did not follow a straight line law, hence a theoretically perfect rendering at one luminosity might be very far out at another, and it was this very difficulty that Mr Bull was attacking so successfully. He suggested the use of a spectrum from a wide slit as a more satisfactory source of mixed colours than a coloured chart.

Mr A. C. JOLLEY could not pass without comment the beautiful and convincing demonstrations which Mr Bull had devised to illustrate his points. The question of filter records intended to follow curves and those having abrupt absorptions was fully discussed in a joint paper of 1904, and experience tended to strengthen the facts therein stated; no matter how tempting filter records following mixture curves might be in theory, the fact that they would in practice work only at one critical exposure condemned them, since in one subject one might have considerable range of light and shade. It would undoubtedly seem, therefore, that filter records which constitute even bands terminating abruptly between prescribed limits, and whose overlaps in the yellow and blue-green are restricted to a region whose component wave-lengths are to constitute the printing colours, would more truly reproduce a subject with considerable range of light and shade than filters whose records possess definite maxima, and there was but little doubt that the correct regulation of this overlap was one of the most important factors in the successful application of the process.

Mr DOWSE.—Changes in hue consequent upon variation in luminosity or illumination could be demonstrated by the following simple experiment:—Arrange two white surfaces, such as small filter papers, in such a position that they are close together, but unequally illuminated by some source of light. If these surfaces be observed through coloured glass from a distance of a few feet, a difference in colour will be manifest. When the more illuminated surface appears yellow, the other surface has an orange tint; if the former be violet, the darker surface becomes bluer in tint. A degradation with a colour nearer the red end of the spectrum seems to take place when the illumination becomes less. Mr Bull had also pointed out that in examining the spectrum of an arc lamp the sodium D-line which was always present appears bright yellow on an *orange* ground.

Dr DRYSDALE said that every one must be struck with the amount of work involved in the preparation of Mr Bull's paper, and with the simplicity and beauty of his demonstrations of the colour sensitiveness of plates. He did not intend to make many remarks on the paper, as pretty well all his experience in the subject had been summarised in an article which he wrote about two years ago. He mentioned that article because at that time the nomenclature of the subject was not very clearly defined, and he had unfortunately employed the term colour sensation instead of colour mixture curves. It was perfectly obvious, as Mr Bull had contended, that the sensitiveness should follow the colour mixture curves. With that correction, he was glad to find that Mr Bull's conclusions verified his own statements, and it was especially interesting to notice that Mr Bull, Dr Clay, and himself agreed as to the inks theoretically requiring to have narrow bands. It was so rare for any two people to agree on anything in three-colour work that an agreement of three was worthy of note.

With Dr Clay, he felt that one of the most important things which Mr Bull had put forward, and which he was perhaps the only one to realise, was the influence of the sensibility curve of the plate for varying exposure. The importance of this could hardly be overrated, as it very strongly modified the theoretical conditions for the best reproduction. In fact, for perfect colour reproduction to be possible the modification of the sensibility of the plates to varying exposures would have to be attempted, and this matter was almost as important as the sensitiveness to colour.

ON THE NICOL PRISM AND ITS MODERN VARIETIES.

A Lecture delivered at the Optical Conference, June 1905,

By Professor Silvanus P. Thompson, F.R.S.

In an assembly of technical opticians there is no need for a lecturer to explain what a Nicol Prism is, nor what its uses. My object is to deal with the development of the subject from the days of William Nicol downward, and to present to you some of the more recent varieties of the apparatus that has become of classical importance.

Ever since the days of Bartholinus, Newton, and Huyghens, the double-refraction exhibited by Iceland spar has been famous; and the recognition that the two "rays" in which the crystal divided any given ray of ordinary light, differed from one another in their properties, led experimenters to seek for some device or apparatus which should transmit one of the two, while blocking off the other. The *double image prisms* of Wollaston, De Senarmont, and Rochon are examples which realise this aim in their several ways, and the prism of Nicol and its congeners really differ from them only in the one essential respect, that one of the two rays is shunted out of the field of view.

Prior to the time of Nicol there was indeed one primitive attempt at the same result, to whom due is unknown to the lecturer, namely, a device in which, by making a short saw-cut extending partially across the crystal through each of the two blunt corners of a rhomb of Iceland spar (each cut acting as a lateral diaphragm), the view angle through the rhomb was narrowed so much as to exclude the ordinary ray. The resulting prism cannot, however, have been very satisfactory in performance. Nicol, by the very ingenious device of inserting obliquely across the rhomb a transparent mirror formed of a film of Canada-balsam, caused the ordinary ray to be totally reflected aside at angles of internal incidence which allowed the extraordinary ray to pass unhindered. This he accomplished by slicing an elongated rhomb (the ratio of the long edges to the short ones should be from 3 to 3·7) obliquely across from one of the blunt corners to the other, polishing the cut faces, and then re-uniting them with the balsam. The process sounds simple, and the particular angle of the section is, qualitatively, immaterial; but it is not at all easy work, as any one who has tried will know well. On looking through any ordinary Nicol prism, towards an illuminated surface such as a white sheet or a white cloud, and turning it about to peer through it in different directions while it is held close to the eye, one at once observes that the useful field of view is a region varying (in ordinary Nicols) from about 15° to 20° in width; being delimited between two slightly curved boundaries, the one a blue band beyond which is a region of darkness, the other a rainbow-tinted set of fringes beyond which there is a region of greater brightness. The blue band marks the limit where the useful (extraordinary) ray vanishes by total reflection for internal incidences, at the balsam film, greater than the critical angle for the extraordinary index. The rainbow-tinted fringes mark the limit beyond which the ordinary ray is no longer eliminated by total reflexion, and the light beyond this limit, containing both ordinary and extraordinary beams, is not polarized.

This is not the place to enter into a discussion of the cause of the blue band and of the rainbow-tinted fringes (really interference fringes) which appear at the limits of the visible useful field of the prism; they have been explained by me elsewhere [1] many years ago.

In the ordinary Nicol prism, when tested by very accurate methods, the direction of the resultant polarization is not absolutely parallel in all parts; perfect extinction of plane-polarized light is not attained all over the field at once, a shadow seeming to move across the field as the prism is slowly rotated.

Now the modern improvements that have been made upon the original design of Nicol may be said to have been directed to some one of the following points:—

1. Procuring a wider angle of useful field.
2. Procuring a field in which the direction of the resultant polarization is more nearly uniform all over.
3. Obviating loss of light by reflexion at end faces.
4. Increasing the actual working aperture for a given weight or given length of prism.

[1] S. P. Thompson, "On Interference Fringes within the Nicol

Constructively these improvements may be classified under the following heads :—

    1. Selection of most favourable orientation of the position to be given to the transparent film.
    2. Selection of appropriate material for the transparent film.
    3. Cutting the end-faces to be normal or nearly normal to the incident and emergent beams.
    4. Finding methods of cutting that will give foregoing advantages without involving too great a waste of spar.

This last consideration has been forced upon the designers of prisms by the increasing scarceness of the material since the closing of the mines in Iceland.

The first points to which attention will be drawn are the numerical statistics about the natural rhombohedra of Iceland spar. The rhombic faces presented by the natural cleavages have two obtuse angles of 101° 55′ each, and two acute angles of 78° 5′ each. The rhombohedron has two blunt summits where three faces (each with the obtuse angle above-mentioned) meet symmetrically. The dihedral angle between any two of the three faces that meet at the blunt summit is 105° 5′. Each of the three faces makes an angle of 45° 23′ 24″ with the axis of symmetry. The dihedral angle between any one of the three top faces and any adjacent one of the three bottom faces is 74° 55′. The axis of symmetry makes an angle of 63° 44′ 50″ with any one of the three dihedral ridges that meet at the summit, or with any one of the corresponding three dihedral ridges that meet at the bottom. The angle which the dihedral ridges between any two of the three faces meeting at the summit makes with the remaining face is 109° 8′ 10″.

The indices of refraction of Iceland spar and of the materials used as transparent films, are as follows :—

		μ	$\dfrac{1}{\mu}$
Iceland Spar *Ordinary Ray* .	Red	1·65308	0·60493
	Yellow (D)	1·65441	0·60444
	Violet	1·68330	0·59407
Iceland Spar *Extraordinary Ray*, at limit . . .	Red	1·48391	0·67386
	Yellow (D)	1·48455	0·67360
	Violet	1·49780	0·66765
Canada Balsam .	Red	1·523	0·654
	Yellow (D)	1·526	0·655
	Violet	1·553	0·644
Linseed Oil . .	Red		
	Yellow (D)	1·485	0·6734
	Blue		
Poppy Oil . .		1·463	0·6835
Balsam of Copaiba .	.	1·549	0·6458
Carbon Disulphide .	Red (C)	1·6209	0·6169
	Yellow (D)	1·6303	0·6134
	Blue (G)	1·6779	0·5960
Castor Oil .		1·490	0·6711
Cinnamic Ether .	Red (C)	1·5530	0·6439
	Yellow (D)	1·5607	0·6407
	Blue (G)	1·6261	0·6150
Monobromnaphthalin .	Red (Li)	1·6482	0·6067
	Yellow (D)	1·6585	0·6031
	Blue (Cs)	1·6939	0·5903

It must be remembered that the value given for the refractive index of the extraordinary ray is its *minimum* value, which it has for rays that cross the optic axis at right angles. If, at the region of the reflecting plane where the extraordinary ray is about to suffer total reflexion (*i.e.* where the blue band appears), the orientation of the crystal is such that the direction of the optic axis is not at right angles to the incident ray, then at that place the value of the extraordinary index will not be a minimum, and reflexion will occur at an angle smaller than the proper and possible limit, and the blue band will encroach more upon the useful field than it need do.

The following are the angles of total internal reflexion in Iceland spar against surfaces of the the materials named for yellow light :—

	Ordinary Ray.	Extraordinary Ray at Limit.
Air	37° 2′	42° 16′
Canada Balsam	67° 53′	none
Poppy Oil	62° 28′	82° 47′
Balsam of Copaiba	74° 34′	none
Linseed Oil	63° 36′	90°
Cinnamic Ether	70° 45′	none

As the index of Canada balsam is higher than the minimum index for the extraordinary ray, it might be supposed that there would be no total reflexion of the extraordinary ray at any angle of internal incidence up to 90°. But, unless the orientation of the crystal is as stated above, the index for the extraordinary ray will not be at its minimum, but at some value between the minimum and the index of the ordinary ray. In point of fact in the ordinary Nicol the angle of internal incidence at which the extraordinary ray is reflected is about 82°10′.

In this connection it may be noted that in Nicol's original paper he spoke only of "increasing the divergency between the two rays," and it was left to Fox Talbot[1] to point out that both rays come through at a certain obliquity, but that one image vanishes by total reflexion *before* the other. Even now there are books[2] which assert that total reflexion of the extraordinary ray does not occur.

The facts were succinctly stated by the writer in 1881 (*Phil. Mag.*, November 1881, p. 350) as follows :—"The refractive index of balsam for light of mean refrangibility may be taken as 1·54, that of the ordinary ray in calc-spar as 1·66, that of the extraordinary ray as 1·487. The reciprocals of these are very nearly in the respective proportions of 65, 67, 60. The extraordinary index, however, is 1·487 only for rays at right angles to the crystallographic axis, these having a minimum, and increasing up to 1·66 for rays whose direction coincides with that of the axis. The ellipsoidal wave-surface of the sheet of extraordinary waves lies partly without and partly within the spherical wave-surface for Canada balsam, while the spherical wave-surface for the ordinary waves lies wholly within. Hence total reflexion may occur for the extraordinary as well as for the ordinary rays ; but of the extraordinary rays only those can suffer total reflexion which are situated in such a direction with respect to the optic axis that their corresponding portion of the ellipsoidal wave-surface lies within the spherical wave-surface for balsam. As the Nicol prism is usually constructed, this limit of possible extraordinary total reflexion occurs for rays (in a principal plane of section) inclined at about 10° [more accurately at about 7° to 8°] to the balsam film, giving rise to the limit of the polarized field marked off by the blue iris."

The ordinary construction of the Nicol prism is readily understood by reference to Figs. 1 and 2. Fig. 1 represents a natural rhombohedron of spar which has been so split off that the length of the

[1] *Phil. Mag.*, 1834, p. 289.

[2] *Vide* Tyndall, *On Light*, p. 125 :—"Now the refractive index of Iceland spar is for the extraordinary ray less, and for the ordinary ray greater than for Canada balsam. Hence, in passing from the spar to the balsam, the extraordinary ray passes from a less refracting to a more refracting medium, where total reflection cannot occur," etc.
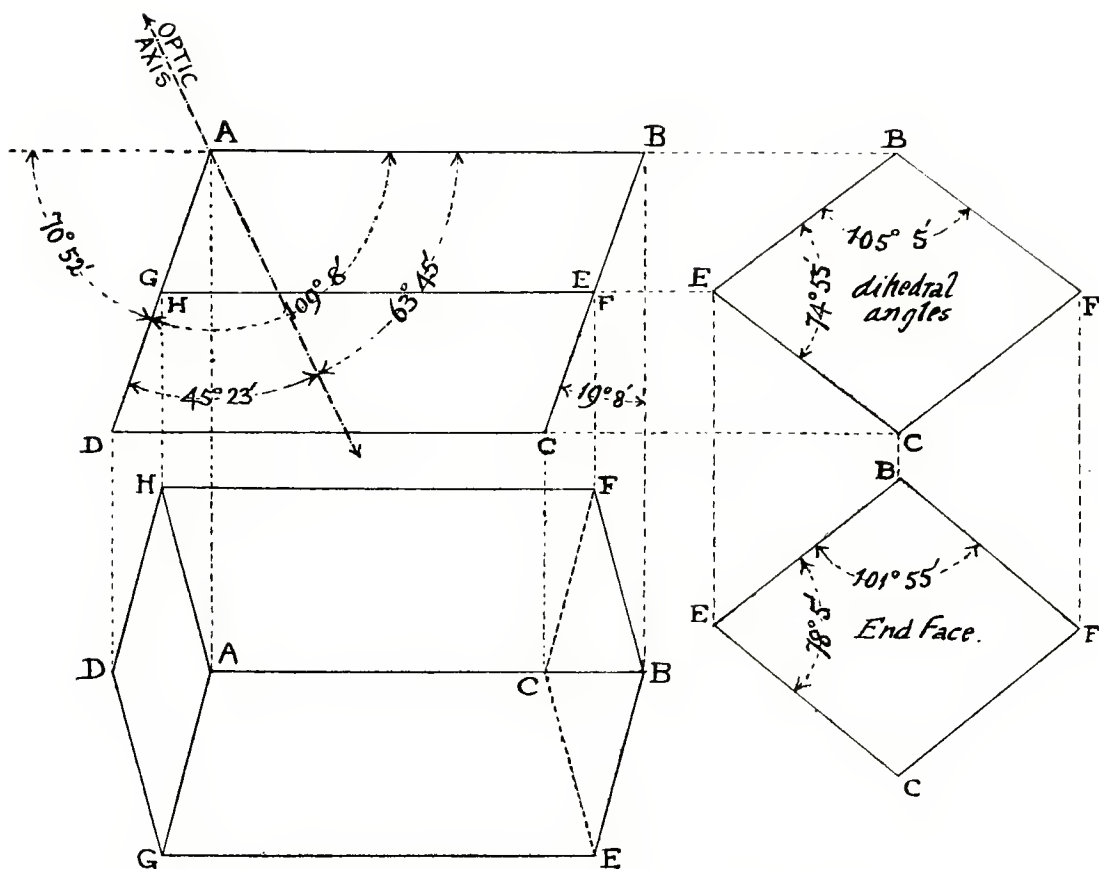
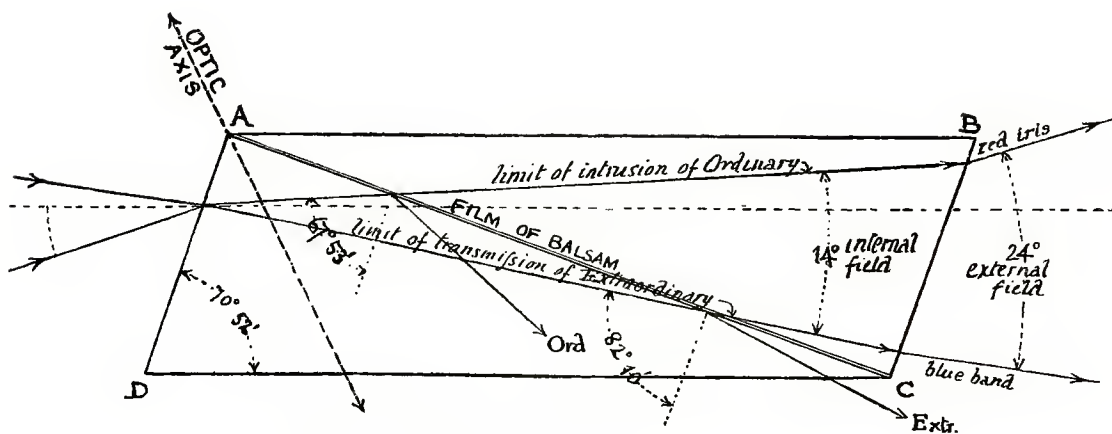Fig. 1. A Rhombohedron of Iceland Spar, and its Natural Angles.

Fig. 2. The Section Plane is in a Nicol Prism.

long edge AB is about $1\frac{1}{2}$ times the length of BC the shorter diagonal of the end face. Then the end faces are inclined at 70° 52′ to the line AB; or assuming the central axis of the light traversing the crystal to be parallel to the long edges (AB, DC, etc.) these end faces are inclined 19° 8′ out of the position for normal incidence. The crystallographic axis makes 45° 23′ with the face AD. To make an ordinary Nicol a piece is required in which the ratio of length to width is about 3. Suppose such a long piece of crystal as represented in Fig. 2 to be sliced through by a plane through the corners A and C, as shown, and the balsam film to be inserted here (shown by double lines). Then this plane will be almost exactly at 90° with the end faces, and will make about 19″ 8′ with the long edge AB. The angle of obliquity between the crystallographic axis and the film will be about 44″ 37′. Now the limiting angle of the ordinary ray is determined by its total reflexion at an internal incidence of 67° 53′ with the balsam film, and hence the limit of useful field where the ordinary ray begins to intrude will be 70° 52′ − 67° 53′, or about 3° aside from the longitudinal axis of the prism. Similarly, the limiting angle for the total reflexion of the extraordinary ray being about 82′ 10°, the limit of the useful field at the blue band will lie at 82° 10′ − 70° 59′, or about 11° on the other side of the longitudinal axis. The angular width of the useful polarized field will therefore be about 3° + 11° = 14°
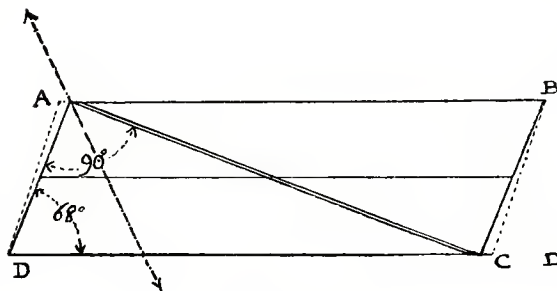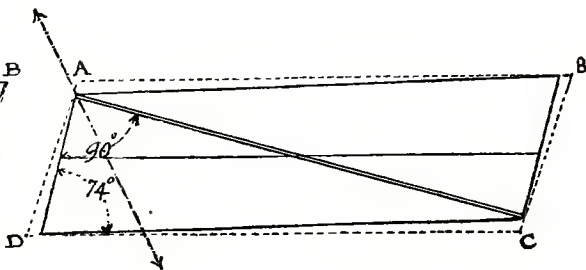
Fig. 3. TRIMMED NICOL.　　　　Fig. 4. AHRENS'S TRIMMED NICOL.

as measured internally; which corresponds to a useful width of field of about 24° measured in the air externally. The middle direction of this useful field is not, however, parallel to the long edges of the prism, but is inclined some 10° or more unsymmetrically. Probably for this reason there arose a practice, apparently introduced by Nicol himself, of shaping the rhombohedron slightly before cutting it across. The end faces which naturally make 70° 52′ with the long edges are trimmed about 3°, to make about 68° with the long edges; and the latter are trimmed off correspondingly, as in Fig. 3. The length is then about 3·7 times the thickness.

In recent years Mr C. D. Ahrens has found it better to trim the spar the other way: the angle of the end faces is increased by $3\frac{1}{2}°$ to $74\frac{1}{2}°$; and the long edges are then trimmed $3\frac{1}{2}°$ to correspond, as in Fig. 4; the prism is then cut at 90° with the end faces. This construction has the advantage of throwing back the blue band, and increases the angular width of field. In some of Mr Ahrens's prisms the trimming angle exceeds 5°.

The Shortened Nicol.—Departure from the exact dimensions or angles above given does not much affect the performance of the prism save in so far as it may narrow the useful width of field or change the obliquity of the end faces. Indeed it is almost impossible to cut any rhombohedral piece of spar across in any oblique direction, and cement together again the cut and polished faces without getting as the result a more or less useful polarizing prism. He trimmed the two end faces of a shorter rhombohedron to 68°, and then made the plane of section 90° to the end faces: later constructors have taken other angles. Nicol himself, in 1839, found that he might use pieces of spar having relatively shorter dimensions. For example, Messrs Steeg and Reuter have for over twenty-five years

furnished shortened Nicols, of which the end faces were natural cleavage faces at 70° 52′; but the plane of the film was cut not at 90° with these, but at about 84°, reducing the ratio of length to thickness from over 3 to 2·84. If cemented with Canada balsam the dissymmetry of the useful field became exaggerated, and it was found preferable to use as cement balsam of copaiba, giving an external visual field 24° wide. The form recommended by Hasert in 1861 is still more shortened, and it would have a very restricted angle of field.

Flat-ended Nicols.—The obliquity of the end faces not only causes a loss of light by reflexion, but occasions a slight displacement of the image as the prism is rotated. To avoid these defects prism cutters have for many years trimmed the end faces to be at right angles to the long edges of the prism.
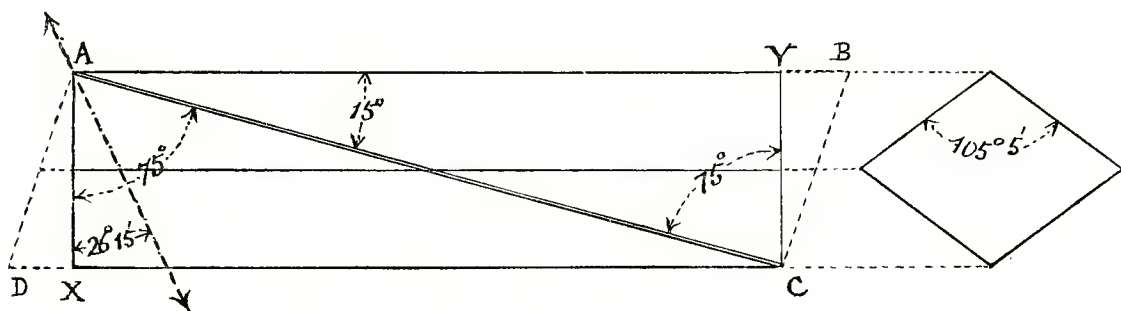
Fig. 5. FLAT-ENDED NICOL.

The angle between the end face and the crystallographic axis is 26° 16′; the usual angle between the end face and the balsam film is 75°; and the angle of field is from 24° to 27′, according to the nature of the cementing substance. Such prisms are sometimes described as *square-ended*, though the actual form of the end face is a lozenge with angles of 105° 5′ and 74° 55′.

Hartnack's Prism.—This prism appears to have been jointly devised by Hartnack of Potsdam and Prazmouski of Paris. In order to throw back as far as possible the limit of the blue band, the plane of section for the reflecting film is in this prism laid exactly at right angles to the crystallographic axis. This involves a great sacrifice of spar, for much must be cut away to procure a prism. Fig. 6 shows the relative sizes of the prism, and of the block of spar from which it is cut. Let a piece of crystal split to have all its edges equal in length be set with its crystallographic axis AC vertical. Let it be sliced off horizontally through the planes *ab* and *cd*, and the top and bottom pyramids be removed. The remaining block is drawn in plan. From it is then cut the rectangular block as shown. The plane of section of the film is at right angles to the axis. Hartnack and Prazmouski worked very scientifically at the question of attaining a maximum field of view, by choice of suitable cements, and by selection of proper slope of end faces. Hartnack gives the following figures :—

	Possible width of field, interior.	Suitable angle between end faces and film plane.	Width of field, external.	Ratio length to width.
Canada Balsam .	20° 54′	79°	33°	5·2
Balsam of Copaiba . .	24° 42′	76°	35°	3·7
Linseed Oil . .	26° 24′	73°	35°	3·4
Poppy Oil .	17° 0′	71°	28°	3·0

Feussner recalculating Hartnack's figures, finds that for linseed oil (in which the extraordinary ray will suffer no total reflexion right up to 90°, that is to its coincidence with the film of oil) the most favourable obliquity of the end face is 76° 5', and that then the width of field in the external air will be 41° 54', with a ratio of length to width of 4·02.   He gives the following table for shorter prisms of similar construction :—

	Angle between end faces and film plane	Width of external field.	Ratio of length to width.
1.	76° 5'	41° 54'	4·04
2.	74° 5'	35° 0'	3·51
3.	72° 37'	30° 0'	3·19
4.	69° 39'	20° 0'	2·10
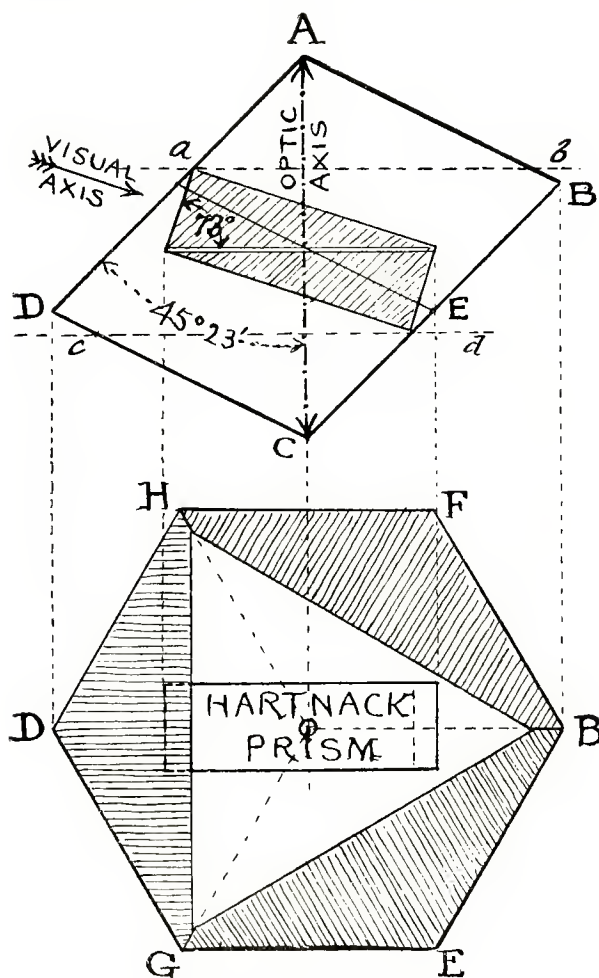
Fig. 6.   Hartnack's Prism.

The Hartnack prisms of commerce correspond very nearly to the second of these proportions.

Foucault's Prism.—In this form a thin film of air is substituted for the film of balsam; with the inevitable result that both rays are totally reflected at not very different angles, the resulting useful field being therefore very narrow. From the table of angles, on p. 218 above, we see that, even with the most favourable orientation of the plane of section, the angle of internal reflexion for the extraordinary ray against an air film cannot exceed 42° 16', while that of the ordinary ray is 37° 2', giving 5° 14' as the maximum internal angle of separation possible. The angle of obliquity of the air film with respect to the axis of vision is given by Foucault himself as 59°, but is variously stated at 58° 8' (Mousson), and even 54° 30', while the inclination of the air-film with the end faces is stated at 51° (Mousson), or as little as even 35°.

The ratio of length to width of the prism was given by Foucault as 1·15, but is stated by others at from 1·528 to 1·16. The useful field of view is usually under 8° wide.

Fig. 7. FOUCAULT'S PRISM.

Square-ended Foucault Prisms.—A square-ended Foucault prism, known as *Hofmann's Prism*, has the air film at 50° with the axis of vision. A further modification due to Soret alters this angle to
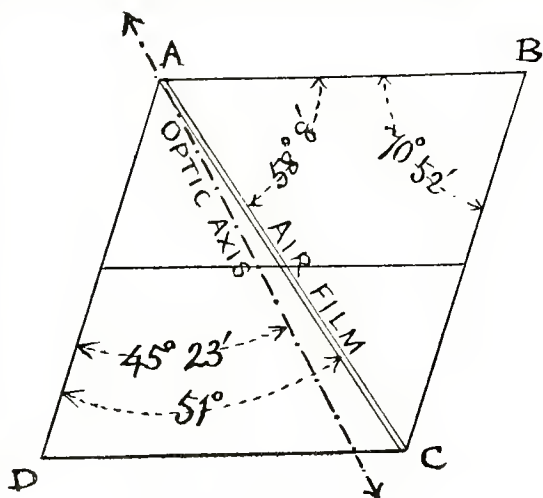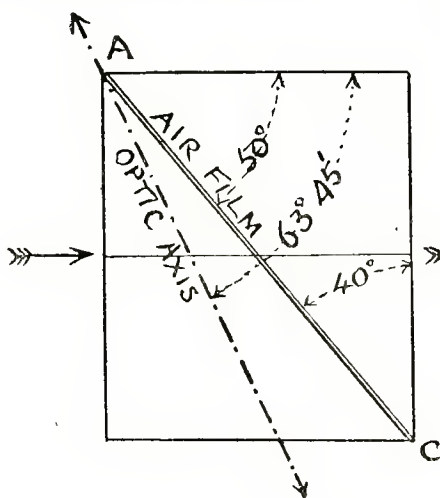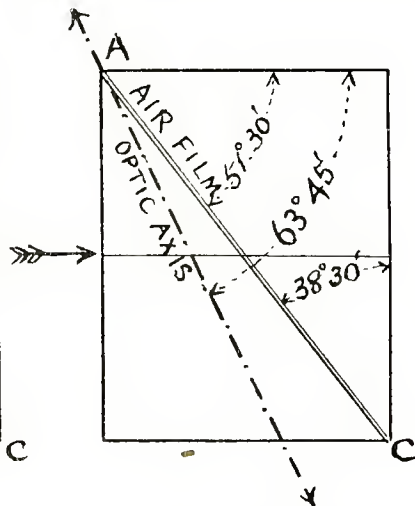

Fig. 8. HOFMANN'S PRISM.

Fig. 9. SORET'S PRISM.

51° 30'. The field of these prisms is between 7° and 8°   Crova (*Journal de Physique*, 1879, p. 175) states that Duboscq also makes these prisms.

Glan-Foucault Prism.—Dr. P. Glan (*Carl. Rep.* 1880) had the idea of making a square-ended Foucault prism with its air film so oriented as to be a principal plane of section, containing the crystallographic axis. The angle between the air film and the end faces is 39° 43'; the useful field width 7° 56'; the ratio of length to width only 0·924.

All the varieties of Foucault prism labour under two disadvantages: they have a very narrow field, and they are liable to give trouble, owing to multiple reflexion taking place in the air film. On the other hand, they use less spar, and are lighter, for a given linear aperture.
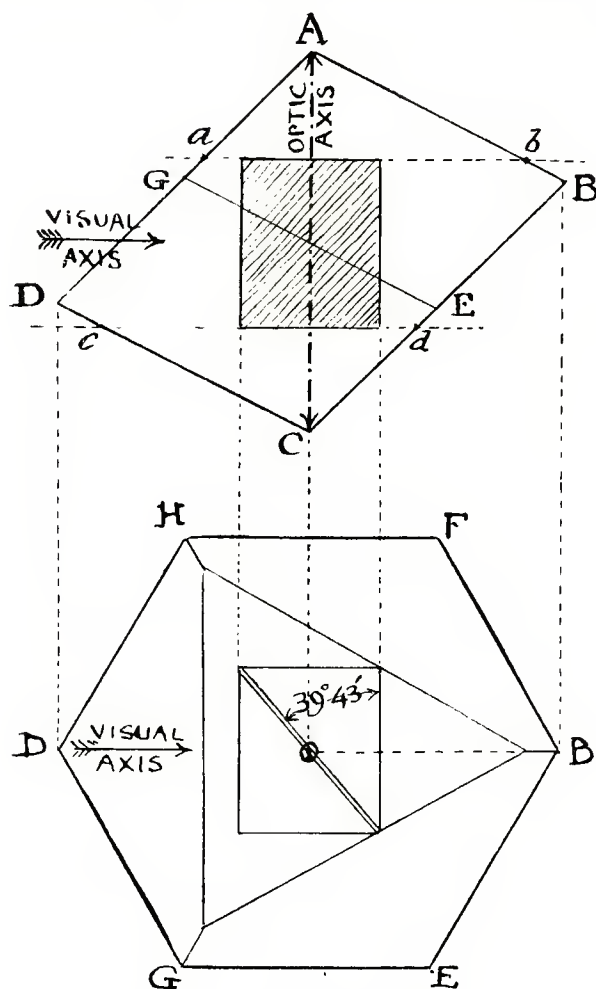
Fig. 10.   GLAN-FOUCAULT PRISM.

Dove's Prism.—This belongs to a different order of apparatus.   It consists of a single uncemented right-angled prism so cut from a block of spa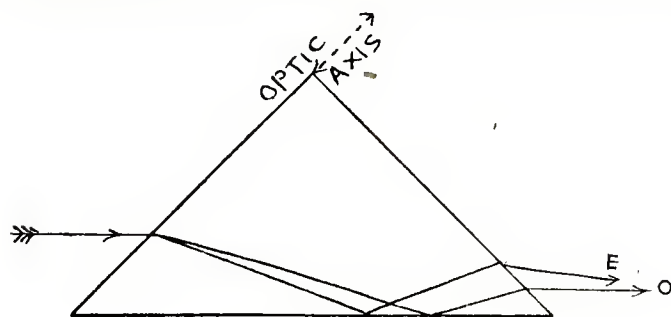r that the crystallographic axis lies in the plane of the first, and at right angles to the second of the two shorter faces. Then, as Fig. 11 shows, a ray parallel to the hypotenuse on entrance is split into two rays, O and E, which makes different angles, but of which O is totally reflected at angles of incidence at which E is no longer totally reflected. The prism becomes therefore a direct vision investing prism for the ordinary ray, but deflects the extraordinary ray. It has but a narrow field, and has

Fig. 11.   DOVE'S PRISM.

not come into any general use.   Grosse has discussed several modifications of this prism.   It cannot well be used as an eyepiece, as on rotating it the image rotates with double the angular displacement.

Jamin's Prism.—Jamin proposed to save spar by reversing the construction and placing a thin slice of Iceland spar obliquely between two triangular prisms, as a matter of fact liquid prisms. The apparatus was constituted by a metal tube closed with thin glass faces at the ends, having a slice of spar, about 2 mm. thick, set obliquely across it, and then filled with carbon disulphide. The spar, to get the best use, must be cut so that its faces are principal planes of section.

Zenker's Prism.—Zenker sought to render Jamin's form more manageable by setting the thin slice of spar between two wedges of flint glass, to which they were cemented by balsam of tolu.

Feussner's Prism.—This prism resembles the last two in having the film of double-refracting material cemented between two wedges of glass. The glass and cement ought to have an index of refraction equal to that of the higher of the two indices of the crystal, and the optic axis of the slice ought to be perpendicular to the planes of its faces. Feussner points out that there are some crystals possessing a greater difference between their maximum and minimum indices than Iceland spar. For example, soda nitrate has $\mu_0 = 1·587$ and $\mu_\varepsilon = 1·336$. This if substituted for calc-spar would give a maximum field-angle of 56° as against 44°, while reducing the length-ratio from 4·25 to 3·34. Potassium chromate has indices of 1·319 and 1·725, with which it should be possible to make polarizing prisms with field-angle over 90°. Dr Feussner also recommends gum-dammar as a cement.

Leiss's Prism.—Leiss proposed in 1897 to take any ordinary Nicol or Hartnack form, and to substitute for the second wedge of spar in it a wedge of identical shape, made of glass having the same index as the extraordinary index of spar.

Von Lommel's Prism. — Independently Von Lommel arrived at a similar suggestion.

Bertrand's Prism.—This is a prism on somewhat similar lines, but requires a cement of the same refractive index as the ordinary ray in spar. He suggests a mixture of albumen and gelatine—which, however, becomes opaque after a time.

Fig 12. FEUSSNER'S PRISM.

Abbe's Prism.—A 60° prism of calc-spar, the refracting-edge of which is so oriented as to coincide in direction with the crystallographic axis, is cemented on each face to a 30° prism of crown-glass; so that the combined prisms present a rectangular section. The ratio of length to breadth is as $2 : \sqrt{3} = 1·15$. It is necessary that both the crown-glass and the cement shall have a refractive index equal to that of the extraordinary ray in the spar. If this is realised, the extraordinary ray passes through as through a homogeneous transparent block. The ordinary ray is diverted aside (as in a double-image prism) by an angle which, for normal incidence, is 11° 40. Hence the useful field has a width of about 23°. With a spar prism of 90° between two glass prisms of 45° the divergence angle between the rays will be 22° 7′. The length-ratio is then, of course, 2.
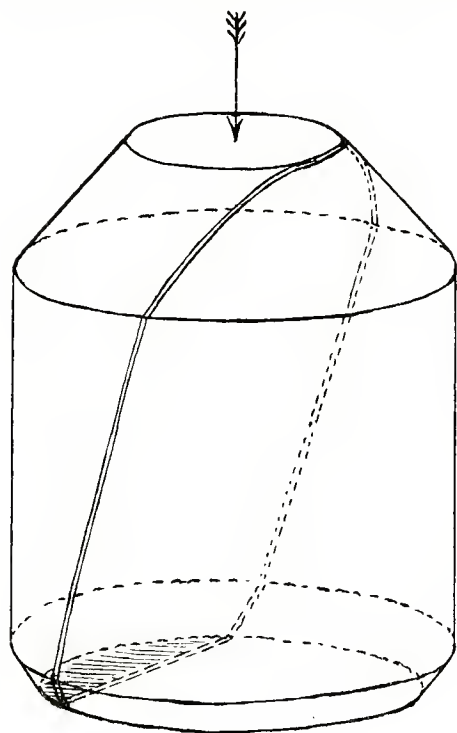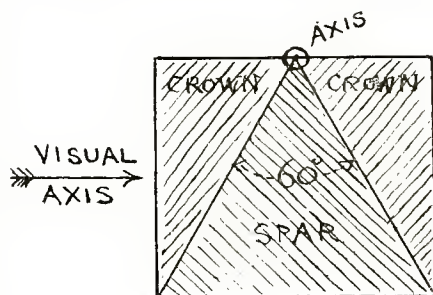

Fig. 13. ABBE'S PRISM.

Researches of the Author.—For over twenty years the author has made attempts to improve polarizing prisms, and has produced a number of successive forms. He published in 1877 a short investigation into the occurrence of interference fringes within the Nicol prism. In 1879 he proposed to replace one-half of the Nicol by a glass wedge of equal refracting power; but the prism so made at that time was not satisfactory, because of the difficulty of procuring a specimen of glass of sufficiently near equality in refractive index to the spar. There was therefore a residual deviation effect, as well as secondary spectrum.

THOMPSON I., September 1881.—In this prism are introduced the principles (1) that the reflecting film lies in a principal plane of section; (2) that the optic axis is at right angles to the line of vision.
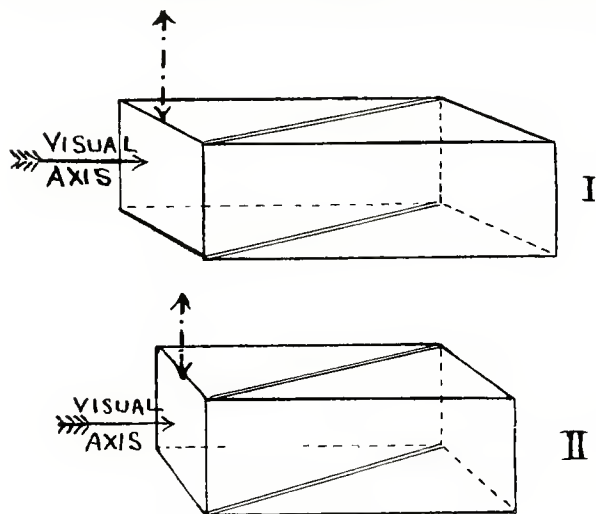

Fig. 14. THOMPSON I. AND II.

In the first prisms cut—some by Mr Ahrens, others by Steeg and Reuter—the end faces were oblique at about 70° to the visual axis. This construction shifts back the blue band entirely, and widens the field to 35°. It was pointed out at the time that this form had the advantage of producing a field in which the rectilinear polarization approximates more uniformly and symmetrically to a polarization in one plane than is the case in the ordinary Nicol.

THOMPSON II.—To obtain fuller symmetry in the distribution of the polarized field, the preceding construction was, in 1882, modified to have square ends. Several prisms, including some in the collection now before the Conference, were made with different materials. The cost of these was in part defrayed by a grant from the Wollaston Fund of the Royal Society. Measurements were also made on a spectrometer of the width of field angle when different cements were used between the same pair of spar wedges. With Canada balsam the angle was 27°, with linseed-oil 34° 42', with poppy-oil 28° 30'. A square-ended Nicol of ordinary form by Hilger, said to be cemented with a mixture of balsam and castor-oil, gave a field of 25°, but was unsymmetrical, the blue band being 9° to one side of the axial direction, the orange band at 16° on the other side. When re-cemented with balsam of copaiba, this prism gave a field of 34° 42', the widths on the two sides being 17° 42' and 17° respectively.

The particular orientation of the crystallographic axis, which is characteristic of both these forms—namely, that it is parallel to the plane of the film, and at right angles to the line of vision—secures the absolute attainable maximum of advantage; and if square-endedness is prescribed for the sake of uniformity of field and minimum loss of light, the only further questions that can arise are (1) the best kind of cement; (2) the best obliquity to give to the film; and (3) the most economical way of cutting the spar. With castor-oil or linseed-oil the extraordinary ray is not reflected at any angle up to 90° incidence (*i.e.* grazing obliquity) with the film, and with linseed-oil the critical incidence for the ordinary ray is 63° 36'; therefore the internal maximum angle of field is 90° – 63° 36' = 26° 24'. If now we add the condition that the middle ray of the field is to be perpendicular to the end face, the latter must be given such an inclination with respect to the film that the angles of incidence for the extreme rays of the pencil right and left of the normal to the surface shall be equal, and that the two corresponding angles of refraction—one ordinary, the other extraordinary—must add up to 26° 24'. It is easy to see that approximately the two external angles will be of the order of 20° to 22° each. Hence we can find the exact value by assuming for one of them—say the ordinary ray—a number of values of incidence

angle above and below this value. From these calculate the corresponding values of the refracted ordinary angle, subtract these individually from 26° 24′, giving the corresponding internal extraordinary angles, which, divided by 1·485, give the corresponding external extraordinary angles.

Ordinary incident .	30°	24°	23°	22	21°	20°	15°
Extraordinary incident	13° 0′	18° 11′	19° 2′	19° 54′	20° 50′	21° 43′	26° 18′
Total external angle	43	42 11′	42° 2′	41° 50′	41° 50′	41° 43′	41° 18′

Hence 20° 55′ on either side of the visual line, giving an external field of 41° 50′ in angular width, is the appropriate value. If the extreme extraordinary ray has an external angle of incidence of 20° 55′, it will have an internal angle of refraction of 13° 55′, and as its direction then coincides with that of the film, it follows that the angle between the film and the end face will be 90 − 13° 55′ = 76° 5′, hence the ratio of length to breadth of the prism will be 4·16.

Independently of the author, Dr R. T. Glazebrook, the President of the Conference, was working at the same subject, and published his results in March 1883 in the *Philosophical Magazine*. Dr Glazebrook attacked the subject analytically, with the view of finding out what orientation would give, on the rotation of the prism about its visual axis, the most uniform turning of the resulting plane of polarization for a conical beam of incident light. The solution led to a resulting construction identical with the author's. Lippich, in 1885, described a square-ended prism having the optic axis perpendicular to the long edges cemented with linseed-oil, and having the film at from 66° to 66° 30′ with the end faces. Its field-angle was bounded between the limits of 3° 30′ at one side, and 4° 20′ at the other side of the visual axis.

THOMPSON III. *Laterally-cut Nicol.*—About the same time Mr Ahrens constructed for the author
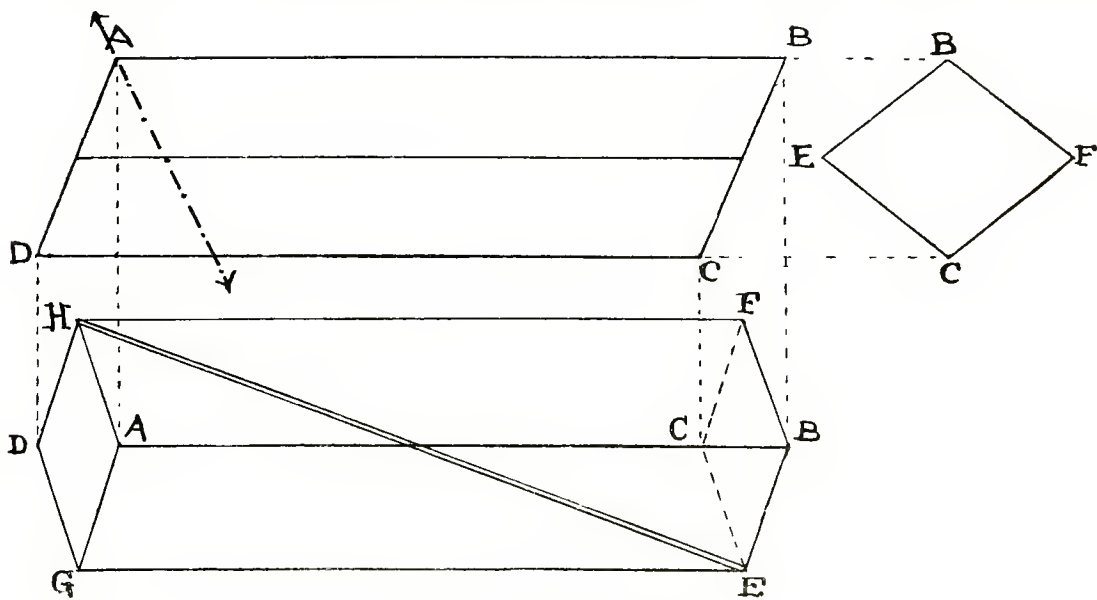

Fig. 15. LATERALLY-CUT NICOL.

a Nicol prism, externally of ordinary shape, but having the plane of the film carried laterally, from the corner H to the corner E. It was hoped that this construction would give a rather shorter prism for an equal angle; but the gain was not great. It was found advantageous to truncate the corners H and E a

little, making the apparent aperture nearly hexagonal in form. No publication was made at the time of this variety.
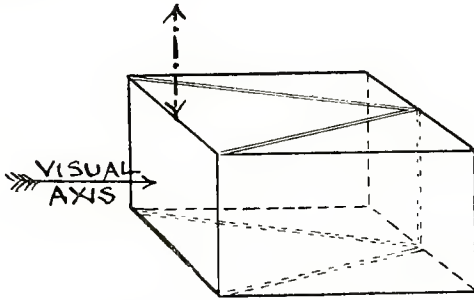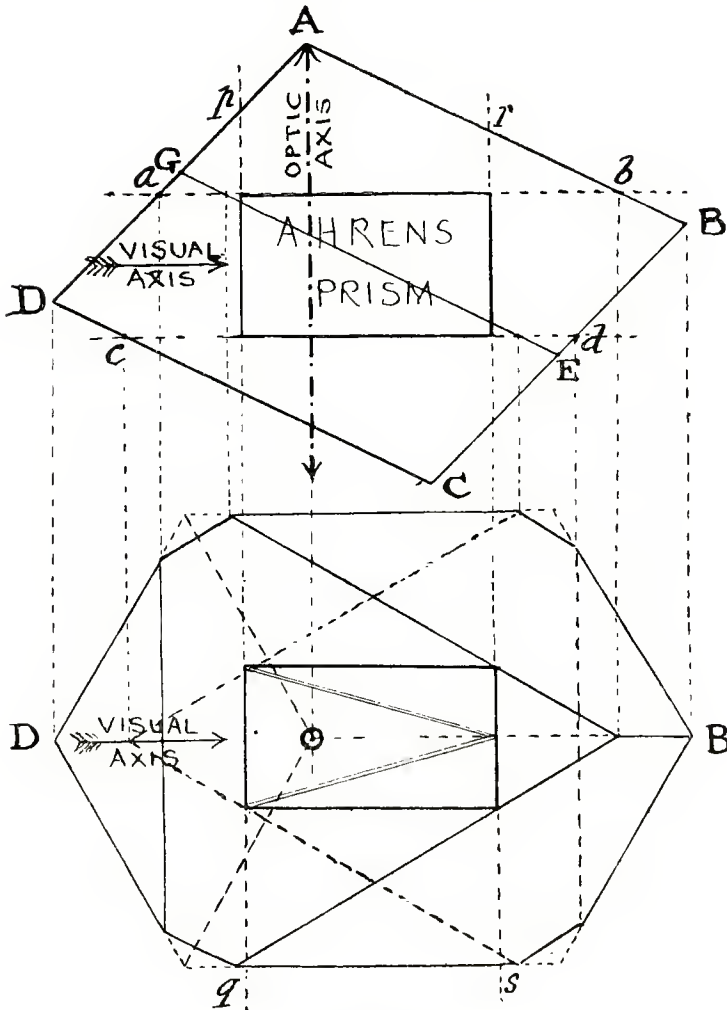

Fig. 16. AHRENS'S TRIPLE PRISM.

Fig. 17. AHRENS'S TRIPLE PRISM.

Ahrens's Triple Prism.— In April 1886 Mr Ahrens described a new prism, which as it embodies the principles laid down for the best orientation of the axis, may be taken as a development in the same line. A rectangular block of square section, having the long sides 1·8 times the sides of the square faces, is cut from spar, so that the crystallographic axis is perpendicular to two of the long faces, so that the end faces are principal planes. This block is then divided into three wedges, as shown in Fig. 16. The planes of section are then polished and cemented together with balsam, and the end faces are polished finally. The ordinary ray is here reflected partly to the right, partly to the left. The useful field angle is about the same as before—26°; but the length for a given linear aperture is reduced to about half that of a flat-ended Nicol. The line of junction across one face does not seriously interfere with the operation of the prism. In practice it is found expedient to cement over this end a thin cover-glass to secure optical continuity. Fig. 17 shows the method of cutting the spar used by Mr Ahrens at this date. The crystal is first truncated by the two planes through *ab* and *cd*, at right angles to the optic axis, then cut through the two planes *pq* and *rs*, which are parallel to the optic axis. From the central block so left the parallelopiped is cut.

THOMPSON IV., June 1886. *Reversed Nicol.*—As the construction found to be theoretically best involved much waste of spar in the cutting, other means were sought of improving the Nicol prism. To bring the film more nearly into the Hartnack position of perpendicularity to the crystallographic axis, the following plan was adopted. The rhomb of spar is about four times as long as broad. The
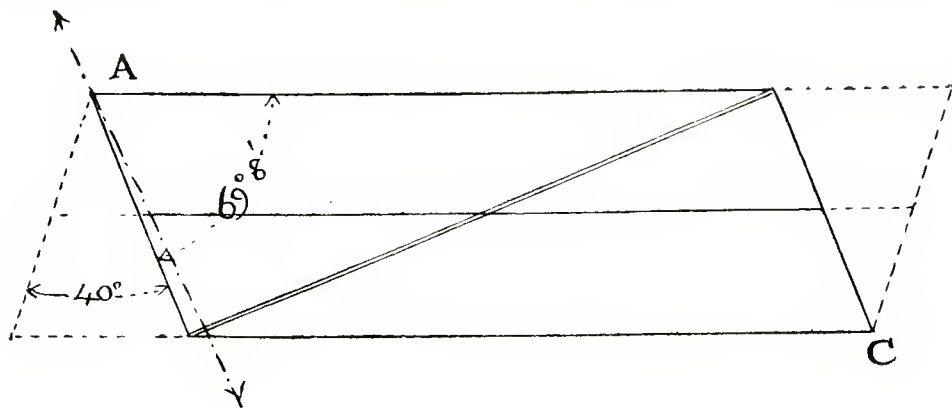

Fig. 18. REVERSED NICOL.

oblique end faces make 45° 23' with the axis, and if they were to be cut off square the amount to be trimmed off is 109° 8' − 90° = 19° 8'. If we trim off double this angle, *i.e.* 38° 16', we shall have new end faces of the same obliquity as before, but reversed, and the new end face will make only 70° 7' with the axis. If we trim of 40°, the axis will then be only 5° 7' away from the end face, and the angle between the end face and the long edges will be 69° 8'. Let this angle be given at both ends, and let the prism then be divided across, as in the Figures 18 and 19, from one of the new blunt corners M to the other N, at right angles to the new end-faces. This gives a prism practically identical in external form with an ordinary Nicol, but its film lies at 84° 37' with the crystallographic axis, so nearly at right angles that there is a considerable gain in angular width of field, as in the Hartnack construction.

THOMPSON V. *Shortened Reversed Nicol.*—The same process of reversal may be applied to a shorter rhomb of spar, though it is less advantageous. Prisms on this and the preceding plan, made by Steeg & Reuter, were shown by the author at the British Association Meeting at Aberdeen in 1886.

THOMPSON VI. *Reversed Flat-ended Nicol.*—The same process of reversal is possible with square-ended Nicols. For, assuming that a piece of spar has been taken with a length-ratio of, say 3, and that its ends have been trimmed off square, it is just as easy to slice it diagonally by a plane of section that
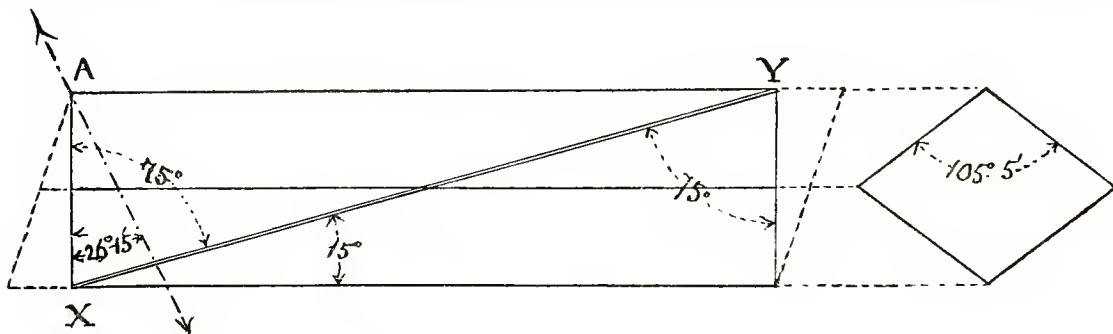

Fig. 19 REVERSED FLAT-ENDED NICOL.

makes 78° 45′ with the optic axis, as by one that makes 48° 45′; the cut being taken from X to Y, as in Fig. 19, instead of the usual A to C.

Ahrens's Method of Spar-Cutting.—Between March and June 1886 Mr Ahrens communicated to the author a method of cutting a crystal of spar, which, while leaving the end faces principal planes of section, would admit of a considerable saving of spar. This is illustrated in Fig. 20. First two principal planes of section *xx* are cut through the two blunt corners A and C. The two pieces so
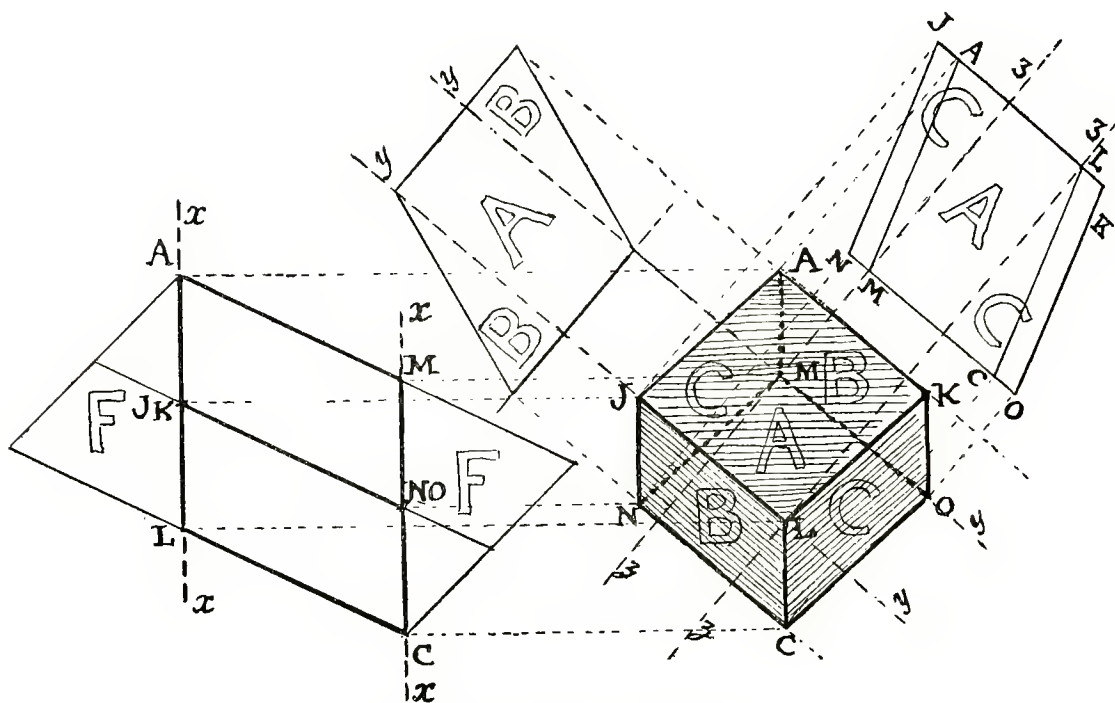

Fig. 20.　AHRENS'S METHOD OF CUTTING SPAR.

removed, marked F and F, may be utilised by polishing their faces and uniting them as a Foucault prism. The remaining block has two end faces, AKLJ and MOCN, each of which is a principal plane of section. Through these face, squarely, two other planes *yy* may then be cut, parallel to the edges AK, JL, etc. Then two other planes *zz*, at right angles both to the *xx* and to the *yy* planes, are cut through the block, as shown. This leaves the following portions :—A rectangular block **A**, two wedges **B**, two wedges **C**, and four corner waste pieces. The block **A** can be utilised as a Thompson prism, or as an Ahrens triple, according to its proportions. The two wedges **B** can be cemented together to make another square-ended Thompson prism, and the two pieces **C** likewise, after suitable trimming. So the crystal yields three square-ended improved Nicols and one Foucault prism, with exceedingly little waste of spar.

THOMPSON VII., November 1887. *Twin-prisms for Polarimeters.*—The author described two forms of twin-prisms. The first consisted of two flat-ended Thompson II. prisms, of rectangular section, having the sides of the section as 2 : 1. These were to be cut on Ahrens's method, which makes the optic axis lie in the end faces at nearly 45° with the sides of the section. Then let one be reversed end for end, so that its optic axis rakes across at about 45° in the reversed inclination. The principal planes of these two prisms will then be nearly at right angles to one another, and the juxtaposed field of vision will be suitable for polarimetry as in the so-called half-shadow combinations. The second form of twin-

prism had the two prisms of the same nature as before ; but after they have been made, a small bevel of about 2° 30′ is ground off from the flank of one of them, and they are again juxtaposed ; so that there is just this same amount of angular displacement between the respective planes of polarization in the two halves of the visible field.   This construction is simpler than that of Cornu for giving a comparison between two fields of nearly equal extinction, and is distinctly preferable in its results to Jellett's prism.

Fig. 21.   Eye-piece Nicol.

THOMPSON VIII., June 1889.   *Eye-piece Nicol.*— This was a special form of square-ended Nicol, with end faces principal planes of section, but using very little spar, as the end nearest the eye might have very small aperture.
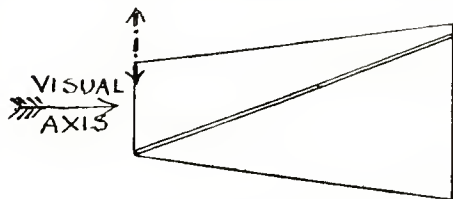
THOMPSON IX., February 1891.   *Spectro-polarizing Prisms.*—These are direct-vision spectroscopic prisms which also polarize the light.   They consist of three prisms cemented together, the two end prisms being of fluor-spar, and the middle one of Iceland spar.   The fluor-spar is of the colourless variety.   The low index of fluor-spar $= 1\cdot4338$ for μ_D, and its remarkably small dispersion, $\mu_F - \mu_C = 0\cdot00446$, render it peculiarly serviceable for this application.   The Iceland spar is cut so that the crystallographic axis lies at right angles to the line of vision.

Fig. 22.   Spectro-Polarizing Prism.

THOMPSON X., 1896.   *Reversed Foucault.*—The same principle of reversion used in Thompson IV., V., and VI., can be applied to the Foucault prism.   Let the end-faces be trimmed off by 40°, and let the remaining block be sliced across at 45 to the new end faces, or 65° 52′ to the long edges ; the faces are then polished and put together again with an air film between them.   The view angle is increased to about 9°.   The pieces cut off at the ends may be also utilised to make another Foucault prism of smaller angle.
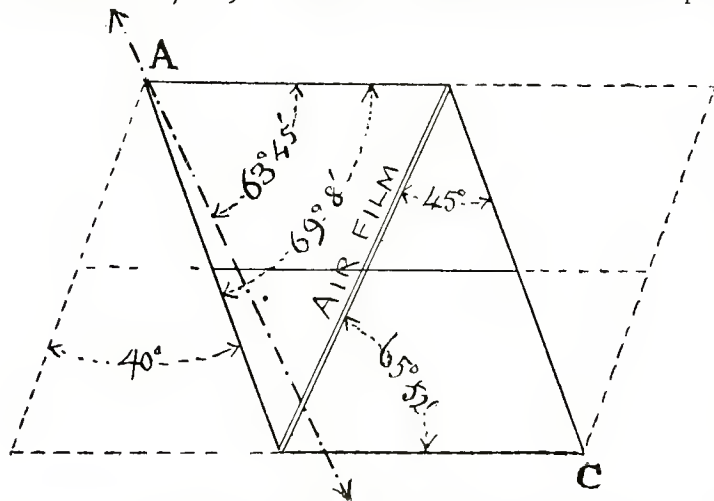

Fig 23.   Reversed Foucault.

THOMPSON XI., 1896.   *Skew-cut Nicol.*—This construction depends on the principle that any plane which passes through the blunt corner, and contains the principal axis, is itself a principal plane of section.   Of all such possible planes one is chosen, see Fig. 24 (plan), passing vertically through AL (axis) and through the natural edge AH ; and a similar one through the corresponding natural edge CE.   The removal of these end pieces leaves two new skew end faces, AQLH and CEMR.   The view on the right represents the block so turned that the end face AQLH is viewed full on.   The next process is to slice the block across from the edge LQ to the edge MR, thus dividing it into two wedge-shaped parts marked **A** and **B**.   These two wedges are then transposed, as indicated by the dotted upper lines, the face LCEQ being cemented to the face HRMA.   The end faces are lozenge-shaped, and they are nearly square to the long edges QM and LR of the resulting prism.
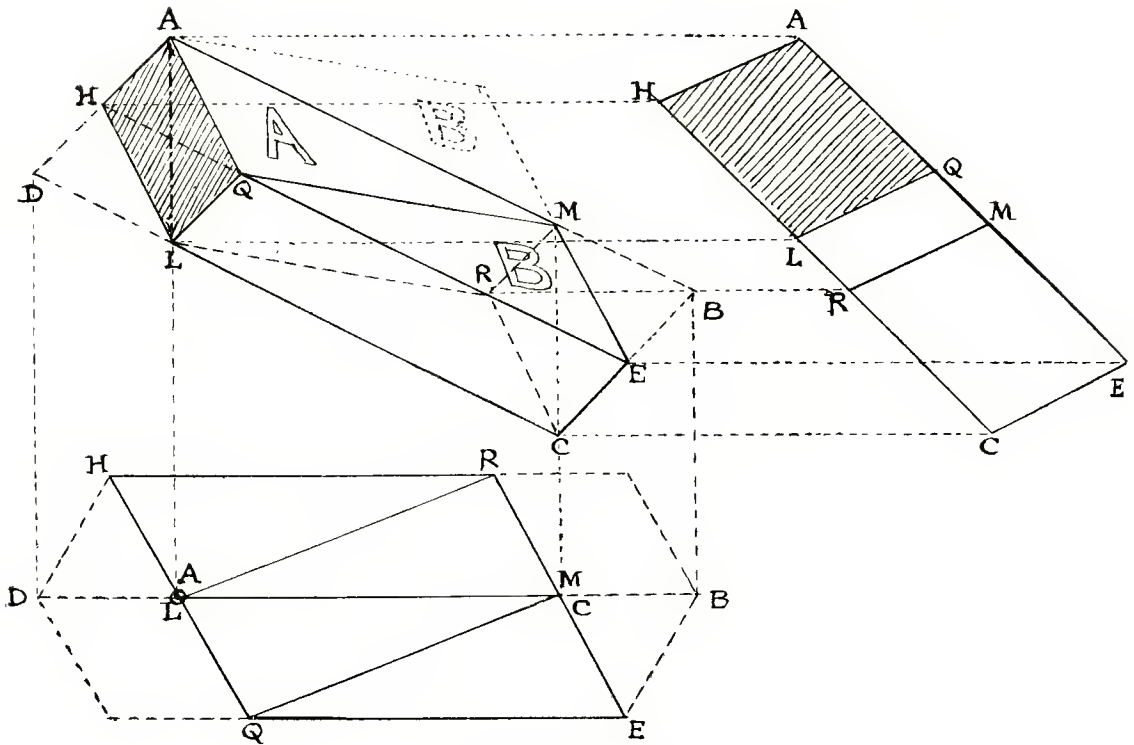
Fig 24. SKEW-CUT NICOL.

THOMPSON XII. *Transposed Nicol.*—This is a compromise form, intended to effect the utmost saving of spar, while nearly attaining the same advantages as those gained in Thompson II. It is difficult to explain the plan without a model. Fig. 25 shows the procedure. Consider a natural block of spar having the natural faces split off so that the edges are about 4 : 5 : 15 in proportion. This piece is represented in the figure as laid down upon its face DGEC. Consider the natural face ABFH. Through the edge AH a vertical plane of section xx is chosen, at right angles to the face ABFH; and the end is thus trimmed off. The new face AHKJ is not a principal plane of section; but it is not very oblique to the crystallographic axis. The other end is similarly trimmed through CE. Then a fresh vertical plane of section yy is chosen, at right angles to this new face, and to the face ABFH. By this plane the block is divided into two wedges marked **A** and **B**, which are then transposed and cemented together, the face DCFH to the face GEBA. This gives a flat-ended prism; and the waste of spar is less than 15 per cent.

An Ideal Prism.—Grosse has made a suggestion of a wholly new kind of prism in which a film of a soda nitrate crystal is mounted between two wedges of calc-spar in such a way that the ray which is ordinary in one becomes extraordinary in the other, and that the spar wedges are so cut that the ray of the extraordinary beam which runs parallel to the film shall have an index of 1·587. The cement may be a mixture of monobromnaphthalin and gum dammar, with an index of 1·58, or of mono-bromnaphthalin with balsam of tolu, with an index of 1·62, as recommended by Feussner. Grosse shows that such a prism, if square-ended, and with the film at 18° 33′ 36″ with the long sides, will give a field of over 60° in width; the length-ratio being 3·53. With a length-ratio of 1·73, and an obliquity of film of 30°, the field is 22° 55′. Probably, on account of the difficulty of procuring a uniform film of soda-nitrate, the ideal prism has never yet been constructed.

Comparative Tests of Prisms—In 1887 Grosse made a number of comparative tests of prisms from various sources. He had at his disposal the following :—An ordinary Nicol with oblique ends, a square-ended Nicol, a Hartnack-Prasmouski, a Foucault, 2 Glans (*i.e.* Glan-Foucault), a Dove, and a cemented prism made in accordance with the principle laid down by the author, and therefore properly described as a Thompson, though called by Dr Grosse a Thompson-Glan. These prisms were examined for their transparency by a photometer, by a method of comparison of extinction, and thirdly by theoretical calcula-tion of the loss of light by reflexion. The figures under Method I. are percentages, those in the next columns purely comparative.

	CLEARNESS.			WEIGHT.
	Method I.	Method II.	Calculated.	
Nicol (oblique-ended) . .	20	...	...	80
Nicol (square-ended)	...	1	1	...
Thompson . .	42	0·93	0·98	12·5
Hartnack-Prasmouski	28	0·89	0·98	25
Dove .	32	0·75	0·83	...
Foucault	30	...	0·64	90
Glan I. .	45	...	0·49	17·5
Glan II.	16	0·59	...	17·5

The figures in the fourth column are the percentages of spar utilised if the prisms were to be cut from equal pieces to begin with; they must not be taken to be the weights for prisms of equal aperture.
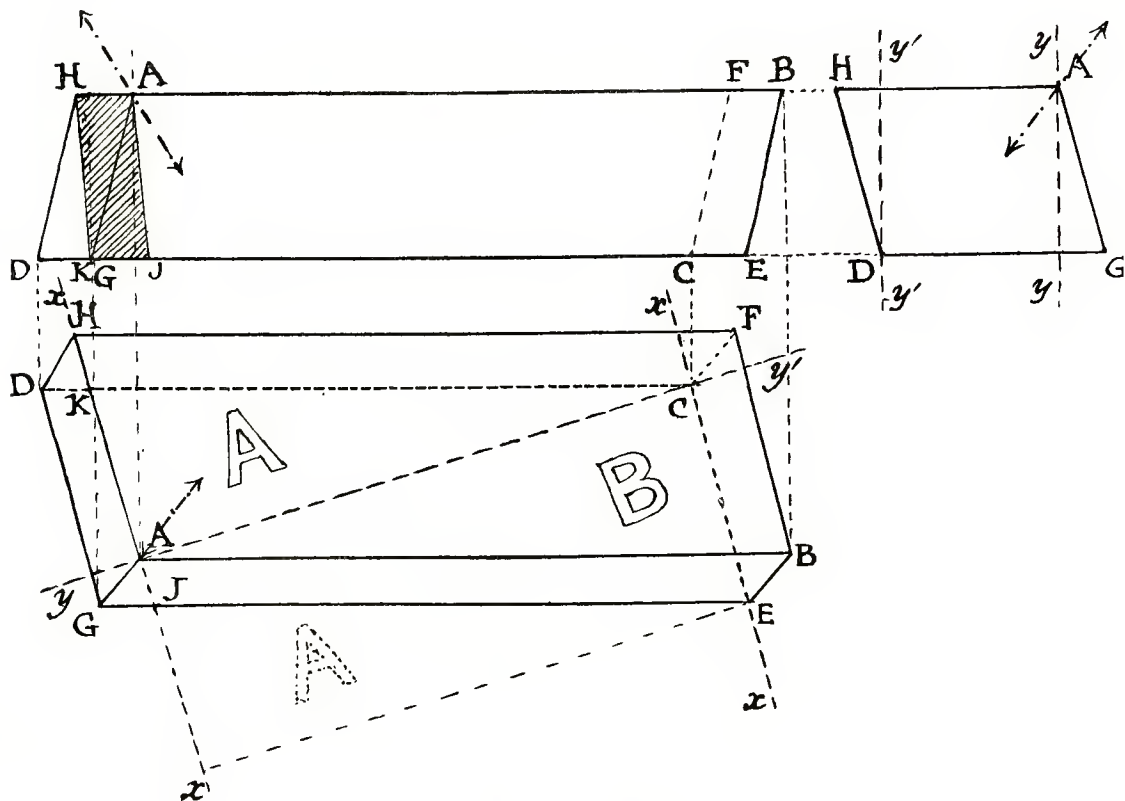

Fig. 25. TRANSPOSED NICOL.

Choice of Prisms for Projection-work.—Since the time of Tyndall it has been fashionable to use two large Nicols for the projection of polariscopic phenomena on the screen. This is, however, quite unnecessary. One large prism, having a clear linear aperture of say $2\frac{1}{2}$ to 3 inches, is advisable *as polarizer* if the object is to be well illuminated, but even one with 1 inch aperture can be made to give fair results if after it there is placed a concave lens of $2''$ focal length and 1 inch aperture, followed at a distance of about 3 inches by a convex of $5''$ focal length and $2\frac{1}{2}$ inches aperture[1]. But, as the light that traverses the polarizer from the lantern condenser is nearly parallel it is not necessary to employ as polarizer a prism of wide field angle, and one of the improved forms of Foucault prism having a field of $9°$ or $10°$ will answer quite well. *As analyser,* however, a wide-angled prism is an absolute necessity, as it must deal with the cone of rays as they leave the projecting lens. Since, however, this cone of rays narrows to a minimum section at a short distance beyond the lens a prism of relatively small linear aperture will suffice; one with a clear aperture of $\frac{1}{2}$ inch will give good results, though $\frac{3}{4}$ inch is to be preferred; but it must be one with a field angle of at least $35°$, and preferably be flat-ended.

Mounting of Nicol Prisms.—The usual plan of mounting Nicol prisms is to enclose them in cork, or to press them with a cork packing into a brass tube. In any case, it is well to cover the longitudinal faces with black varnish to absorb the ordinary ray and prevent internal reflexions. For the same reason these long faces should be left rough-ground, and not be polished. Lippich proposed as a rational method, to employ a special black varnish of lamp-black mixed in some medium of a refractive index approximately equal to that of the ordinary ray, such as gum aloes, or balsam of tolu. It is usual in the construction of very large Nicols to truncate the corners of the prism, making it either octagonal or unequally hexagonal in periphery; such forms can be mounted in tubes of lesser diameter than would otherwise be necessary.

Substitutes for Nicol Prisms.—Owing to the cost of large Nicols for polarizers in lantern work, various substitutes have been proposed. The bundles of glass plates set in an elbow tube are not satisfactory. A better device is that of Delezenne, in which a large total-reflexion prism of appropriate angle is used to project the light from the condensers at an incidence of $57°$ upon a plate of black glass, or preferably upon a plate of glass blackened at the back, and covered in front with a single sheet of thin patent plate glass. The presence of the latter though it renders the polarization slightly less perfect increases the illumination. The Rev. P. Sleeman has found a piece of silvered mirror glass to form an effective substitute for the rather heavy reflecting prism: and when used thus as illuminator the slight reflexion from the first surface is no disadvantage.
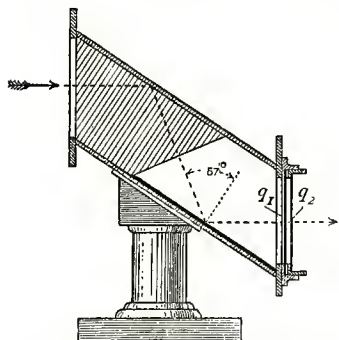

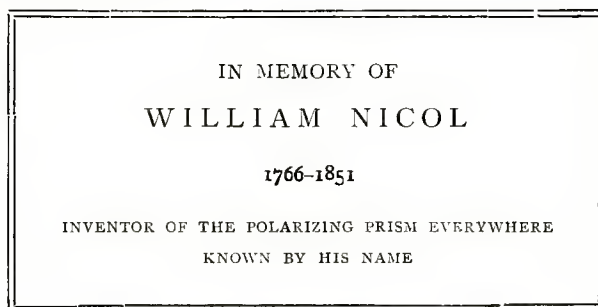Fig. 26. Delezenne Polarizer fitted with Optical Rotator.

Optical Rotator.—But with such apparatus there is the objection that the polarizer cannot conveniently be rotated about its axis as a Nicol can, and must be left in a fixed position. To obviate this objection the author devised an *optical rotator,* an instrument which rotates the plane of polarization to any desired angle. It consists simply of two clear plates, q_1 and q_2, in Fig. 26 of quarter-wave mica. The first of them is mounted with its axis at $45°$ to the plane of polarization of the polarizer, and is fixed on at the end of the apparatus; the second one, q_2, is mounted in a revolving frame that can be turned by a worm-gear and handle to any desired position in its own plane. The first converts the plane-polarized light to circularly-polarized: the second reconverts the circularly-polarized light to plane-polarized, polarized at an angle that depends solely on the position of the optic axis of the second mica plate. This apparatus is constructed by Messrs Newton & Son.

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[1] On the difficulties involved in use of auxiliary lenses to widen the beams, see Crova, *Ann. de Chim.* (5) xxii. p. 881.

An Illustrative Model.—An instructive model of the part played by a transparent mirror may be made by mounting within a rectangular box of sheet glass, sloping at an angle of 33° (and therefore giving an angle of incidence of 57°) a single sheet (or preferably two thin sheets superposed) of thin "patent-plate" glass. This apparatus is inexpensive and light to handle. It is a working model, because it actually polarizes by refraction the light transmitted through it.

WILLIAM NICOL.—Any account of the development of the Nicol Prism would be incomplete if it did not contain a word as to the inventor whose genius first led the way. Strange to say, there is no mention of him in the *Dictionary of National Biography*. William Nicol of Edinburgh was born in 1766, and died on 2nd September 1851. He was at one time assistant to the blind lecturer on Natural Philosophy, Henry Moyes, who did much to popularise science in the North. He was himself a teacher of Physics in Edinburgh, where he lived a very retired life. He was admitted in 1838 to the Fellowship of the Royal Society of Edinburgh. He was living in 1847 in Inverleith Terrace, where he was visited by the then youthful Clerk Maxwell, on whom this visit exercised a stimulating influence. Nicol worked much at optics, and constructed many prisms with his own hands. The small original prism now exhibited is marked by him "W.N. Aet. 79." A number of prisms of Nicol's own make, and a small brass bust of Nicol, were in the possession, about twenty years ago, of the late Mr J. M. Bryson, optician, of Princes Street, Edinburgh. A memorial tablet to Nicol, drawn up by the late Professor Tait, marks the spot, in the north-east corner of Warriston Cemetery, where he lies buried. The inscription on it runs as follows :—

IN MEMORY OF

W I L L I A M   N I C O L

1766-1851

INVENTOR OF THE POLARIZING PRISM EVERYWHERE
KNOWN BY HIS NAME

The memory of one who did so much for optical science ought not to be let die.

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BUSINESS MEETING OF MEMBERS.

FRIDAY, 2nd June 1905.

THE PRESIDENT IN THE CHAIR.

The PRESIDENT, on opening the meeting, stated that the first business was to consider the future of the Convention.

The matter had been discussed by the Executive Committee, and a resolution would be moved by the Hon. Secretary.

Mr F. J. SELBY (Hon. Secretary) moved—"That the Executive Committee of the present Convention think it desirable that steps should be taken with a view to the holding of further Conventions in future years. They desire to recommend to the General Meeting of the members of the Convention that a Permanent Committee be appointed, to consist of the present Executive Committee and others to be added, and that to this Committee be entrusted the task of fixing the date for the holding of the next Convention.

"The Committee would suggest that a general instruction be given to this Committee that the members of the Convention think it undesirable that the next Convention should be accompanied by an Exhibition on the lines of the present Exhibition, but that the chief business should be the holding of meetings for papers and discussions, and the receiving of reports on specified subjects, and would ask the Permanent Committee to consider whether the next Convention should be held some time next year or at a later date. The Executive Committee think it would be undesirable at present to recommend the holding of an Exhibition similar to the present one at a shorter interval than three years."

Sir W. ABNEY seconded the motion.

Dr WALMSLEY, in supporting the motion, pointed out the necessity for a Permanent Committee to carry on the Convention work.

Mr DIXEY suggested that a modified Exhibition might accompany the next Convention meeting.

Mr KNOBEL thought that it would be unwise to resolve on holding a Convention next year, but that the Permanent Committee should have full powers to do so.

Mr HYATT WOOLF said it had been represented to him that the purely optician's side was not well represented this year. Possibly a sectional exhibit might be organised for another meeting, or an annual conversazione might be possible. The question of foreign instruments should also be considered.

Dr DRYSDALE thought it was undesirable to give instructions to the Committee which would prevent the holding an Exhibition next year.

It would not be desirable to have an annual exhibition, but a representative one to include foreign exhibits might be possible next year.

Mr ROSENHAIN suggested some of future meetings might be held outside London.

Mr CHALMERS thought more active country members of the Committee would be desirable.

The PRESIDENT moved, as an addition to the motion, that the present officers be requested to act till the Permanent Committee appoint its officers.

The motion, as amended, was carried.

Mr HYATT WOOLF, in proposing that the thanks of the Optical Convention be given to the Catalogue editors and sub-editors, pointed out the permanent value of the Catalogue, and the obligation under which the Convention was to all who had assisted in its preparation.

Carried by acclamation.

Sir W. ABNEV, in moving a vote of thanks to the exhibitors, said he had been astonished at the excellent Exhibition that had been held.

Carried.

Mr KNOBEL, in moving that "a cordial vote of thanks be given to the Trustees of the Northampton Institute for their liberality in lending the rooms of the Institute for the purposes of the Exhibition and Convention," referred to the difficulty the Convention experienced in obtaining suitable rooms, and the very generous offer of the Trustees to allow the Convention the gratuitous use of their fine hall and various rooms in the building.

He called attention to the very valuable assistance given by Dr Walmsley, and coupled his name with the vote of thanks.

Mr HORACE BECK seconded the motion, which was carried unanimously.

Dr WALMSLEY, in returning thanks, said he would have great pleasure in transmitting the resolution to the Governing Body of the Institute, who were keenly interested in the success of the Convention.

Mr CONRAD BECK moved the following resolution, arising out of the discussion on Dr Walmsley's paper.

"That the Optical Convention hereby expresses its cordial approval of the project for founding an Optical Technical Institute for the training of opticians in the scientific principles of optics and their technical applications, which it regards as a matter of industrial importance to the nation; and in view of the backward state of optical teaching in this country, it urges the London County Council to push forward as a matter of pressing need the foundation of such an Institution on the lines of the scheme which was under the consideration of the late Technical Education Board."

Mr H. L. TAYLOR seconded the motion.

Mr HYATT WOOLF referred to the deputation which had waited on the Technical Education Board.

Mr BECK explained that a trade deputation had waited on the old Technical Education Board, not the London County Council, and had been favourably received, and thought it would be of great advantage if the resolution were adopted.

Dr WALMSLEY said the Technical Education Board had commended the scheme to the favourable consideration of their successors.

The motion was carried unanimously.

On the suggestion of the President it was resolved that the exact method of forwarding the resolution was left to a small sub-committee, consisting of Mr Beck, Dr Walmsley, and the President.

On the motion of Sir W. ABNEV, seconded by Mr KNOBEL, a hearty vote of thanks was accorded to the President.

On the motion of Mr CONRAD BECK, seconded by Dr WALMSLEY, a hearty vote of thanks was accorded to the Hon. Secretary, Mr F. J. Selby.

Mr SELBY, in returning thanks, moved that the thanks of the Convention be accorded to Dr Walmsley, Mr Knobel, Mr Chalmers, Mr Rosenhain, Mr Salt, and Mr Redding for their assistance.

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